

OBSERVATIONS

ON THE

NATURE AND CURE OF

CALCULUS, SEA SCURVY, CONSUMPTION,
CATARRH, AND FEVER:



TOGETHER WITH

CONJECTURES UPON SEVERAL OTHER
SUBJECTS OF

PHYSIOLOGY AND PATHOLOGY.

BY THOMAS BEDDOES, M. D.

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TO THE
DISCOVERER

OF THE VIRTUES OF
*VEGETABLE ALKALI, SUPERSATURATED
WITH CARBONIC ACID,*

SIR,

IT has been frequently with great confidence affirmed, that our acute pains are of short duration. A very slight acquaintance, however, with the tremendous catalogue of human maladies, will satisfy us that this is the vain aphorism of a sophist, more anxious to place words in opposition, than to observe the course of nature. Our excruciating diseases are, if I do not compute very much amiss, remarkable for length of paroxysms, and for frequency of recurrence; while in those of a different character, languor and depression are scarce less intolerable than the most intense pain.

I hope, and I believe, that this mighty mass of evil will be gradually diminished, and finally disappear from the face of the earth. We are just beginning to catch a glimpse of the laws of animal nature; and now, when the human mind seems, in so many countries, about to be roused from that torpor, by which it has been so long benumbed, we may reasonably indulge the expectation of a rapid progress in this, the most beneficial of all the sciences. An infinitely small portion of genius has hitherto been exerted in attempts to diminish the sum of our painful sensations; and the force of society has been exclusively at the disposal of Despots and Juntos, the great artificers of human evil. Should an entire change in these two respects, any where take place, every member of society might soon expect to experience, in his own person, the consequence of so happy an innovation; and should the example be generally followed, there is no improvement in the condition of the World, for which we might not hope from the bloodless rivalry of nations.

From

From Chemistry, which is daily unfolding the profoundest secrets of nature, and, among the rest, the delicate play of living machinery, your example alone would justify us in entertaining the most sanguine expectations: since the earliest discoveries in that department of chemistry, which has been so successfully cultivated by BLACK, CAVENDISH, PRIESTLEY, SCHEELE, and LAVOISIER, suggested to you a safe and efficacious remedy for one of the most frequent, painful, and hopeless of diseases.

Much as you have contributed, by the frank and disinterested communication of your discovery, to obliterate one of the darkest shades from the prospect of life, your name is, I suspect, scarce known beyond the narrow circle of the practitioners of medicine, except, perhaps, to a few among those who are indebted to you for ease and health. Such is the inattention of mankind to their best benefactors! and so entirely have fatal illusions preverted our moral sentiments! I cannot hope to add much to your reputation;

reputation ; but by attempting to diffuse more widely the benefit for which mankind are originally indebted to you, I may perhaps afford you gratification.

That the former part of the following pamphlet will do some good, I am confident ; though I do not believe that alkaline medicines will relieve calculous disorders under every form. Those disorders, beside the different seats they occupy in different persons, appear also, from the analysis of various calculi, to be liable to considerable variation in their nature.

The speculations that follow, will, perhaps, appear to you too remote from application, and my hopes of the future improvement of medicine too high-flying.

It is, I am sensible, but a poor expedient, to lay one's self out for the praise of ingenuity by proposing projects which are in no danger of being disgraced by trial ; nor have I ever much regarded medical observations,

vations, that are of no other use than to be read. But this, I can assure you shall not be the case here; and the more I reflect, the more confident I become, that an easy and convenient method of offering phthical patients a chance of recovery, which has never yet, upon any probable grounds been offered them, will shortly be contrived. For *typhus*, if the light that is now dawning upon physiology and pathology does not present objects to me under very illusive forms, we shall not fail to strike out an almost infallible method of cure; and this method, I think it probable, will extend to the scarlet fever also; which is perhaps the most formidable among the acute diseases of this climate. In the treatment of fevers we have, it is true, learned to avoid some fatal mistakes of our ancestors; but we can boast of little else. In those cases in which alone there is, perhaps, occasion for the interference of art, art seems almost impotent: from attention to the single circumstance of debility, I imagine, that patients are often drenched with wine and opiates, till they are stimulated to death. If I have
imputed

imputed the debility to its real cause, our chief aim should be to restore the principle of excitability ; and stimulants should in the mean time be administered with a more sparing hand. Perhaps, when the proper method of restoring this principle shall have been devised, *extraordinary* stimulants will become unnecessary. The Materia Medica was once supposed to contain distinct specifics for the diseases of each separate organ ; it is now regarded as little else than a collection of stimuli ; so that medicine is become the art of administering drams. Hence it can often only amuse or palliate, and must sometimes injure, by forcing into motion, constitutions already too much worn. How would our resources be multiplied, if we could give excitability or life, as well as stimulants ! “ But “ is so salutary a revolution in medicine “ possible ? ” I do not know ; but is it not worth while to enquire ?

I am, SIR,

Respectfully your's,

THOMAS BEDDOES.

Oxford, }
30th July, 1792. }

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however feeble, on which the wearied spirits of the patient may rest, and defers a little that season of settled gloom when Futurity has nothing to promise to Hope.

There exists, probably, no human malady, not even the jaundice, consumption, ague, cancer, or dropfy excepted, for which so many whimsical and nugatory means of relief have been proposed, as for the stone and gravel. Besides an infinity of inefficacious simples, the whole series of remedies, from the warm goat's blood of Alexander Trallianus, the pounded glass of Baricellus a Sancto Marco, & the essence of pigeon's dung of Johannes Poppius, the *quinta essentia urinæ humanæ* of Fabri, the *spiritus microcosmi e stercore humano* of another chemical or alchemical

§ *Incredibile nec fortè unquam tentatum remedium*, says Dr. Siebold. Yet the powder occasionally found within the cavity of stones is still used as a remedy for calculous complaints in our chalk counties; I met with an instance of this kind within these few days. It was probably conceived, in both cases, that one kind of grit would draw or drive the other out of the body.

doctor,

doctor, down to the bearberry of De Haen, at once afford a proof of the inefficacy of each particular medicine, and of the prevalence of a disorder which could enforce so much attention, and suggest so many extravagant projects.

From the testimonies that have fallen under my observation, I can collect, that during the former part of the present century some approaches had been making towards a remedy, which, whatever may be its mode of operation, or precise degree of efficacy, is undoubtedly capable both of relieving that pain, which renders the disorder so formidable, and of suspending the progress of the disease itself. As early as the year 1721, Robinson proposed salt of tartar, among other things, as a solvent for the stone *. In disorders of the urinary organs, whether arising from concretions or not, Hoffman praises the efficacy of the hot alkaline springs of Germany, as

* Treatise of the Stone and Gravel, 1721.

well as of the salt obtained from the waters of Carlsbad. At a later period, alkaline, substances enriched the English empirics, and obtained the commendation of Hartley, Whytt, Kirkpatrick, De Haen, and other physicians of great celebrity. It appeared, indeed, that the concretions were not really dissolved, even in the more favourable cases; yet the pain was permanently relieved. Every physician is acquainted with the very curious facts on this subject related by Whytt, and more especially by De Haen *. Yet, notwithstanding some partial success, these caustic materials were afterwards generally

* See the first volumes of De Haen's *Ratio Medendi*. The case of that patient who had swallowed eight hundred quarts of lime-water in half a year, and who continued free from pain for three years, though he had still a stone in his bladder, is very remarkable. The ninth case, related in the "*Account of the Efficacy*," &c. affords another such instance of palliation. The patient appears to have been kept more than tolerably easy by the supersaturated solution for two years and an half; yet, from the concluding expression of his letter, "*I am seldom troubled with any pain*," I infer that the disease still existed in the bladder.

laid

laid aside. They were, however, laid aside with regret, since this inference seemed to be warranted by the whole sum of facts that much benefit might be derived from them, provided their collateral bad effects could be obviated. This conclusion appears evidently to have rested upon the mind of Professor Bergman, who, as far as this disorder is concerned, may be quoted as an authority in medicine : in a paper published in 1776 he has these words—“ *hoc unum addo, calculi analysin chemicam medicæ arti haud parum utilitatis polliceri, calcis vivæ aquam et lixivium causticum alkalinum calculo mederi experientiâ constitit. Idem vero si ignotum hætenus fuisset ex ipsâ calculi mixtione nunc inventâ et detectâ intelligeretur.*

It was reserved for a respectable member of the medical profession, still living, to engage the modern chemistry in the service of medicine, and realize a project which now seemed to be relinquished in despair. This gentleman's reflections were quickened by his own feelings, and in

1778, after having been for eighteen years subject to severe nephritic paroxysms, he began to take a solution of fixed vegetable alkali, supersaturated with carbonic acid, or fixed air. This medicine very soon relieved his calculous symptoms, and, as it appears from the account of his case, has kept him free from pain for ten years, one slight attack excepted, which is ascribed to the discontinuance of the medicine for several weeks. Perhaps it might be serviceable to mankind, if medical practitioners, attentive to the progress of science, and capable of combining ideas, were from time to time to be seized by those diseases for which remedies are still wanting. The narrative of this case, with that of upwards of twenty others, is contained in a pamphlet, now well known under the title of "*An account of the Efficacy of Aqua Mephitica Alkalina, &c. in Calculous Disorders.*" Experience has since amply confirmed the virtues of a medicine, which, I apprehend, may be freely taken without danger, and even without inconvenience, except in a few rare

rare instances, and which seems to have deserved the singular praise of equalling the expectations raised by the person who first proposed it. The method of preparing this medicine is as follows: Dissolve two ounces and a half (troy weight) of dry salt of tartar in five quarts (wine measure) of soft water; after stirring the water, and then suffering it to stand long enough for the substances generally precipitated from water by fixed alkali, and the residuum of the salt of tartar itself to subside, pour off the clear solution, and place it in the middle vessel of Parker's apparatus for impregnating liquids with fixed air, and expose it for forty-eight hours to a stream of that elastic fluid. Of the liquor, from twelve to twenty-four ounces have been taken every day by different persons afflicted with various calculous complaints, and always, except in one instance, with the desired effect, after it has been continued some time.

One might perhaps, be disposed to wish that this remedy could be prepared with
less

less trouble and attention ; but the great desideratum is a cheap, safe, and efficacious formula, adapted to the poor, who are by no means exempted from calculous disorders ; for when the high price and brittleness of the apparatus is considered, and when we likewise take into the account the necessity of constantly continuing the medicine, in order to prevent the return of the disease it will appear probable, that the poor are not often likely to reap the full benefit of the discovery.

In the year 1786 or 1787, a person belonging to the medical profession, and much afflicted with the gravel, complained to me that he was unable to persevere in the use of aqua mephitica alkalina, on account of the great dizziness it always occasioned *.

I was

* Dizziness with a degree of intoxication, is a very common effect of liquors containing carbonic acid. Pyrmont water, which contains of carbonic acid air considerably more than a quantity equal to the bulk of the water itself, occasions a glow, exhilaration, and con-

I was led by this intimation to reflect upon the subject, and after some time fell upon what indeed was abundantly obvious; a formula of which I think myself fully warranted in asserting, that it is extremely beneficial in calculous complaints, and that it may, without injury, be taken in very large quantities, and continued for a great length of time. Its simplicity and its cheapness are its great recommendations. I cannot determine, for want of comparative ob-

fusion of ideas similar to that which follows the use or abuse of spirituous liquors. This effect is so common as to have given rise to a particular term, *brunnen rausch*. A spring of the same quality, which the Tartars, on this account, call *the well of drunkenness*, is mentioned by one of the late travellers into Siberia. Some bottled liquors, I suppose, in part, owe their intoxicating power, and more especially the suddenness of their effect, to carbonic acid. This acid is produced in the bottles by a slow continuance of the vinous fermentation; and therefore these liquors will contain more alcohol when they are ripe, than at the time of bottling; but I do not imagine this difference will account satisfactorily for the prodigious difference of their effect upon the head. Sparkling Champagne probably owes its exhilarating power to the same cause.

servations, whether it is inferior to the more operose preparation in efficacy ; and how much inferior ; there can be no reason to suppose that it is superior : and were both equally easy to procure by all patients, I should not think it worthy of public notice.

The formula I have employed for two or three years past is as follows : take natron or sal sodæ in crystals, pound it coarsely, and expose it to a warm dry air, till it entirely crumbles into a white powder ; make this powder into pills with soap or any other cement ; aromatics, extract of bark, &c. may be added ; but I have never found any addition necessary ; a quantity of soap, rather more than equal to the weight of the calcined alkali, is necessary to make it into pills. *

In

* I now see that Dr. Donald Monro (*Materia Medica*, article Alkali, 1788), had an idea of giving fossil alkali in pills, in calculous cases, “ It may,” says he, and this is all he says, “ be made into pills or bolusses,
“ mixed

In order to expedite the expulsion of the water of crystallization, the alkali may be spread out before a fire, or the vessel containing it may be placed in boiling water, and the pounded crystals stirred till they have lost rather more than half their weight : the recent crystals contain indeed sixty-four parts of water in an hundred ; but unless kept close, they lose part of this water, and it is prudent not to carry the calcination too far, if artificial heat be applied ; nor should the heat exceed that of boiling water, lest any of the volatile acid should be expelled ; of this powder, from one to two scruples taken every day has generally af-

“ mixed with some powder of liquorice root, by means of gum arabic mucilage, or conserve.” From several articles of that work I had, perhaps hastily, concluded that it fell short both of modern chemical, and medical knowledge, and this passage had escaped me. I suppose the author, as he gives no intimation of the contrary, means to propose the crystallized alkali for pills. I have often found the pills apt to fall to pieces, when the water of crystallization is not sufficiently expelled ; I should think this would happen still more, when none is expelled.

forded relief in less than three weeks ; and in no case but one, out of more than twenty that have fallen under my own observation, have they failed to perform every thing which could be desired from medicine, except eradicating the tendency to form calculous concretions, to which no known remedy has the smallest pretensions.

I might perhaps safely trust to the above-mentioned pamphlet, as bearing abundant testimony to the efficacy of alkaline salts ; and confidently appeal to future experience in confirmation both of the power of sal sodæ in the form prescribed, and of its harmlessness ; at all events it will be unnecessary to particularize slight cases, which have always at once yielded to the remedy. The following, in which the symptoms were either of very long standing, or extreme severity, will, I hope, be sufficient to procure a trial to the medicine.

I.

MR. WILLIAM RUSHTON, of the Wyke, near Shifnal, Shropshire, had been harassed for a considerable number of years by excruciating pains in his loins, attended with occasional sickness, and an almost total inability to stoop; the pains were accustomed to spread in all directions, and severely to affect his head. He had at different times discharged much gravel; his urine formed depositions, was often extremely offensive, and full of mucus; he was at times afraid to discharge it, so much were the passages irritated, and so intense the pain succeeding the evacuation. In July, 1787, he began to take a drachm of chrysalized fossil alkali, dissolved in a quart of water, every day. In a few days he felt relieved, and in less than a month seemed, as he expressed it, *to be quite another man*. I have seen him repeatedly since; his sufferings from his complaints have been very inconsiderable; but having sometimes neglected the

the

the medicine for months together, he has felt some stiffness rather than pain across his loins, which has immediately been removed by a repetition of the alkali.

The solution sometimes produced a slight nausea, against which his dread of the pains determined him to bear up. The pills have never been attended with the slightest unpleasant sensation. During his long experience he observed, that stale (acid) beer never failed to bring on a severe paroxysm. Several other persons have repeated the same observation. On the presumption arising from this information, I have always enjoined abstinence from malt liquor in that state; no other particular restriction of diet has appeared necessary, yet the inhabitants of cyder countries, as I have been informed upon inquiry, are remarkably free from this disease. Do the native and acetous acids differ in their effects? I should imagine not. The speedy effect of sour beer seems to shew that it does not act by producing new concretions, but by some irritating power.

power. Would cyder affect a person subject to calculous paroxysms in the same manner? Diuretics, as diluted spirits, generally do mischief: the same remark, I think, occurs in the pamphlet quoted above. May, 1792, Mr. K. gave me the usual favourable account of himself.

II.

BRAMAN, a workman in the foundery of Mr. Dearman, of Birmingham, was become quite emaciated, and unequal to his labour, from a gravelly complaint, under which he had laboured many years. He had long been accustomed to discharge concremented matter, and small stones; his urine deposited an incrustation, and soon became foetid; the pains at his loins were intense, &c. &c. He began to take the solution as before in October 1787, found relief in a few weeks, and soon considered himself as radically cured. Notwithstanding my repeated admonitions, he has at times neglected to continue the medicine, and has had returns
of

of his symptoms. I never have seen an instance which more evidently showed how soon we forget the most acute pain.

Having so frequently experienced the efficacy of the medicine, he now chooses to suffer the disease to return in a slight degree, and then for two or three days to take a *handful* of pills; the consequence of which is a discharge of gravel, after which he feels no farther inconvenience for months. I have not been able to dissuade him from this violent method of treating himself; he has not, however, suffered from it.

My friend, Mr. I. Dearman, in a letter dated May 2, 1789, gave me the following account of one of his relapses, for he has so often relapsed and recovered, that his case alone is equal to half a dozen, in proof of the power of the remedy: “ He began
 “ with half an ounce of sal sodæ, with as
 “ much common soap as with the addition
 “ of a little gum arabic made sixty pills.
 “ On second day (Monday) he took four,
 “ increasing

‘ increasing the number every day, till
‘ yesterday he took twelve, without the
‘ least affection of the stomach or increase
‘ of urine; nor can he give any other
‘ account of their effect, than that the
‘ pain is removing very fast, and he does
‘ not doubt but a few more pills will cure
‘ him. I have not been without an apprehension of danger from his taking them so
‘ largely—he treats my remonstrances very
‘ cavalierly, and says he should not mind
‘ taking a box full of such pills. He does
‘ not recollect any increase of urine when
‘ he was taking the medicine before.’ Mr.
Biddle, chemist, of Birmingham, in a letter dated June 1792, informs me, that ‘ Bramah
‘ and Wilks, at the foundery, have constantly taken the pills, when they felt the
‘ complaint coming, and have as uniformly
‘ been relieved.’

III.

WILKS, a fellow-workman with Bramah, and not quite so terribly afflicted

with nephritic symptoms, found perfect relief from the pills. After every other disagreeable feeling was removed, he continued for some time to have a starting of one of his thighs. Suspecting this to arise from some cause of irritation lodged in the urinary passages, or at least to have some connection with his calculous disorder, I advised him to persevere steadily in the use of the pills; and he has since informed me, that he is no longer disturbed by this involuntary motion of his muscles. He also informed me, that he had cured two or three other persons with his spare pills, of which he seemed very desirous to know the composition, having some idea, as I suspected, upon the strength of his experience of their virtues, of trying his fortune in the practice of medicine.

IV.

DECEMBER, 1788. John Bucknall, labourer, of Kemberton, near Shifnall, Shropshire,

shire, has had the gravel for thirty years, and very violently for the last six or seven years. In harvest the pains at his loins have always been most severe, especially when he was reaping or mowing, a kind of labour which his disorder has often rendered him incapable of following. The harvest beer is seldom without some degree of acidity, to which he imputes, in a great measure, the severity of his sufferings at that particular season. The attitude of a mower or a reaper may, however, also account for this aggravation. He was directed to take a drachm of crystals of sal sodæ dissolved in three half pints of water. I believe, however, he much exceeded this dose, as the common people are so apt to do. In three days he perceived an increase of his urine, and a great deal of red gravel came away. He had now the sensation of a large lump in his right groin, which in about nine days totally disappeared.

January 11, 1789. He considered himself as totally cured. He was ordered to

take every day, for a month, a scruple of the salt dissolved in water, at two doses, and afterwards to repeat the medicine in the same dose, one week at least in every month; and if he found the quantity too small to prevent the return of his disorder, to regulate the treatment of himself according to his feelings, which a little experience would soon enable him to do.

I did not see him again till May, 1792, when he told me that he had long totally discontinued the solution, and that he had been perfectly free from any gravelly symptom till within a few weeks, when he perceived a stiffness in his loins, and some other indications of a return of his disorder. I recommended to him the pills, but have heard nothing further concerning him. I met with one patient who would not exchange the solution for the pills. He is the only person I have seen, who thought the taste of alkali not disagreeable.

V.

THE case of Mr. Roe, timber-merchant, at Newport, in Shropshire, was remarkable, on account of the violence of the symptoms, and the extreme irritability of the patient's habit. He complained of violent pains across his loins, accompanied with sickness and vomiting, and intolerable sufferings immediately after the discharge of his urine, and a constant irritation of the bladder and urethra. He had from time to time discharged much gravel, and many calculi as large as horse beans. He was not able to ride without making bloody water. More than one of the former patients had mentioned this circumstance; but I believe the coffee-coloured is often mistaken for bloody urine.

Early in 1789 he took half a drachm of crystallized fossil alkali, with three or
four

four grains of cream of tartar * every day for three weeks. His pains were len-

* I then thought that the addition of a little cream of tartar to the alkaline solution, at the time of taking it, would gradually extricate some fixed air in the stomach and secure the saturation. I even supposed that I might, by a few experiments, discover such a proportion of these ingredients as would effect, without any apparatus, a supersaturation of the alkali that should remain uncombined with the acid of tartar. But the pills superseded the necessity of further researches. On the addition of five or six grains of cream of tartar to a scruple, or a scruple and a half, of sodæ, dissolved in twelve or fourteen ounces of water, closing the mouth of the phial, and inverting it in water, the alkaline solution acquires a much less disagreeable taste than a mere solution of equal strength has, at the moment of unclosing the phial, an effort of protrusion is felt, and a few air bubbles ascend. The neutral salt thus formed would itself be probably beneficial; I cannot believe, at least, that the carbonic acid contributes, by any direct operation, to the cure or palliation of the disease, since caustic alkaline substances clearly seem, if they could be safely taken, to have equal efficacy with the carbonated; and whatever be the effect of this acid, some of those other acids, that are easily dislodged from alkalis, would produce it equally. A respectable observer (Menghini, *Comment. Bonon.* v. 61, &c.) recommends Rochelle salt in calculous cases.

sibly

sibly diminished, and the pills were now substituted in the place of the solution. In a letter dated March 23, 1789, he describes himself as taking the pills: ‘And
‘ tolerably well all the time, with now and
‘ then some little pain, after which a small
‘ quantity of small gravel comes away.’ In a letter, dated 12th April, 1789, Mr Jones who attended him, says, ‘There is no doubt
‘ as to Mr. Roe’s case; I am very happy
‘ to inform you that he continues much
‘ better.’ He took the pills regularly, felt from time to time some slight pains or intimations of pain, after which some gravel was discharged. In April, 1792, he called upon me to inform me that he had enjoyed ease and a good state of health since I had heard from him last.

VI.

THE follow ingletter contains an account of a rare irritability of the urinary organs. The medical reader will certainly adopt the
writer’s

writer's opinion, that it is not a calculous case. I ought perhaps to have suppressed the conclusion of the letter ; but as the fervour of expression, doubtless, corresponds to the pain the writer was once accustomed to suffer, and the relief he afterwards experienced, it is properly to be considered as descriptive of the case, especially as I have not the satisfaction of knowing the person who gives so much credit to the medicine :

‘ SIR,

‘ At the request of my brother-in-law,
 ‘ Mr. Biddle, I take the liberty of inform-
 ‘ ing you of the effect which the alkaline
 ‘ pills, recommended by you, have inva-
 ‘ riably produced upon me ; but it is ne-
 ‘ cessary to premise, that my case appears
 ‘ to me of a very dubious nature, as both
 ‘ the quantity and quality of the substance
 ‘ which I sometimes void by urine are in-
 ‘ sufficient to convince me that my com-
 ‘ plaint is the gravel. But of that Sir,
 ‘ you may better form an opinion, when I
 ‘ mention,

‘ mention, that a quantity of wine, by no
‘ means intemperate, generally produced so
‘ much pain in my kidneys, that to stoop,
‘ or in any manner bend my back, was
‘ impossible ; nor could I easily turn myself
‘ in bed while this pain lasted, which
‘ mostly lasted a week or two, though by
‘ drinking plentifully of diluting liquors I
‘ made water without difficulty ; yet the
‘ last few drops, and sometimes the quan-
‘ tity of a tea cup full, were very bloody,
‘ and came from me with excruciating
‘ pain ; but I never discovered any gravel,
‘ and it appeared to me more like the mu-
‘ cus of the bladder : however, it more
‘ often happened that I had this fixed and
‘ intense pain in the small of my back,
‘ without voiding any thing extraneous in
‘ my urine ; and I am still very liable, after
‘ the use of wine, to returns of it , but since
‘ Mr. Biddle has recommended it to me
‘ to try your pills, I have never failed, by
‘ using them for a day or two, or at farthest
‘ in the course of a week, to obtain effec-
‘ tual relief.

‘ These, Sir, are all the particulars of
 ‘ my case, which is not important enough
 ‘ to merit much attention, nor perhaps
 ‘ will it help much to establish the fossil
 ‘ alkali as a specific for the gravel, but to
 ‘ me it is a valuable discovery, and I have
 ‘ no doubt, when it is generally known,
 ‘ but it will be beneficial to thousands. I
 ‘ beg your pardon for the prolixity of this
 ‘ trifling detail, and earnestly desiring that
 ‘ you may obtain from others the praise
 ‘ and gratitude you have won from me,
 ‘ and that all your labours may be crowned
 ‘ with success, to the promotion of your
 ‘ own fame, and the happiness and im-
 ‘ provement of your fellow creatures, I
 ‘ beg leave, Sir, to subscribe myself,

‘ Your much obliged and

‘ very humble servant,

‘ *Lombard-street,* }
 ‘ *June 26, 1792.* }

‘ D. LLOYD.

Dr. BEDDOES,
Oxford.

The relation of the other facts that have fallen under my own notice would be useless repetition. All the patients for whom I have directed the fossil alkali have been past, and most of them considerably past, the prime of life. Calculi are sometimes found in the bladders of children, but the nephritis calculosa seems to be one among the evils almost peculiar to declining age. Yet the fossil alkali, whatever were the infirmities of those who took it, and in some instances they were very great, has always been perfectly harmless, and, but in one case, decisively beneficial in its effects.

The following communications will probably be considered as strong additional proofs of its efficacy :

‘ DEAR SIR,

‘ I HAD the favour of your letter, and
‘ am happy to hear it is your intention to
‘ give the world your sentiments upon
‘ the efficacy of sal sodæ in calculous dis-
‘ eases.

‘ eases. I am so well convinced of its
 ‘ good effects, that I look upon it as a
 ‘ valuable acquisition. As I write chiefly
 ‘ from memory, I shall not be able, pro-
 ‘ bably, to furnish you with all the cases
 ‘ that have fallen under my observation,
 ‘ but the few I have recollected you may
 ‘ depend upon the accuracy of. The pa-
 ‘ tient, whom you enquire after, took it but
 ‘ a few days, lost his pain, and has since
 ‘ had no return.

‘ Your thoughts upon sea scurvy will
 ‘ be interesting to every one, but parti-
 ‘ cularly so to our sailors and naval practi-
 ‘ tioners. I have met with a few cases (I
 ‘ think three) in this neighbourhood, but
 ‘ they were soon cured.

‘ I am your obedient servant,

‘ J. JONES.

‘ *Newport,* }
 ‘ 25. Feb. 1791. }

‘ C A S E

‘ CASE I.

‘ JANUARY, 1789, N. N. had been for
‘ several years afflicted with gravel, had
‘ frequently violent fits of pain, and for
‘ some weeks past has scarcely ever been
‘ totally free ; has with vast difficulty voided
‘ several large stones. He took fifteen
‘ grains of sal soda, with one of magnesia,
‘ and crystals of tartar, twice a day for a
‘ fortnight ; afterwards about seven grains
‘ of the dry powder of sal soda, with as
‘ much soap formed into pills, for a fortnight
‘ or three weeks longer regularly. He soon
‘ discovered a large mucous and very ropy
‘ sediment in his urine, and a great abate-
‘ ment of pain ; his fits returned at much
‘ longer intervals, were shorter, and much
‘ less violent, and the stones he voided ap-
‘ peared half dissolved, which before had
‘ been hard and rough, and when dry
‘ seemed to be covered with a fine soft
‘ powder. In six or seven weeks he be-
‘ came so entirely free from his disease,
‘ that

‘ that he omitted his medicine for some
‘ months, and has since had recourse to it
‘ occasionally only for a fortnight at a
‘ time. He has never since had a severe
‘ fit, but now and then, perhaps once in
‘ three or four months some slight pain,
‘ and then voided softened stones, more
‘ like pieces of hard clay than calculous
‘ concretions.

‘ C A S E II.

‘ MR. S. a farmer, who had lived
‘ freely, had been long tormented with the
‘ frequent passing of stones from the kid-
‘ neys. I was twice called to him, when
‘ they were so large as to stick firmly in
‘ the urethra, and were removed with con-
‘ siderable force by a pair of forceps, con-
‘ trived for the purpose, upon the principle
‘ of Smellie’s midwifery forceps

‘ He had taken various medicines with-
‘ out any good effect, and upon that ac-
‘ count

‘ count was with difficulty prevailed upon
‘ to try the *sal fodæ*. In April, 1789, I
‘ give him the dry powder with soap (ʒij.
‘ of each in sixty pills) of which he took
‘ two twice a day for near a month, from
‘ that time he has totally discontinued
‘ them, and when I last saw him (Oct.
1790) he had never had a return of his
‘ complaint that he thought worthy of
‘ attention.

‘ CASE III.

‘ IN January, 1790, I was called to a
‘ farmer at some distance from me, who
‘ had for some years suffered much from
‘ a stone in the bladder. He informed me
‘ that he had taken a variety of what are
‘ called solvents, without the least advan-
‘ tage. He had now been for some time
‘ confined to his room, and had scarce
‘ any respite from pain. Under such cir-
‘ cumstances I saw no prospect of relief,
‘ but from the operation, which I strongly
‘ recommended

‘ recommended to him. He could not
‘ be prevailed upon to submit to it,
‘ but was very desirous of trying any
‘ medicine I should propose. The extra-
‘ ordinary efficacy of the sal sodæ I had
‘ seen in the last case, induced me to try
‘ its solvent powers in this.

‘ He began upon it immediately, and
‘ took the usual dose twice a day for six
‘ months regularly. In the space of a few
‘ days his pain abated ; in a month he was
‘ able to walk about his farm ; and in the
‘ course of the summer walked several
‘ times ten or a dozen miles in the day.
‘ He still continued the pills, but with less
‘ regularity, sometimes omitting them for
‘ three or four weeks ; and when I last saw
‘ him, which was in October, remained more
‘ comfortable than he had been for the last
‘ two or three years.

‘ CASE IV.

‘ B. a neighbouring farmer, about 75,
‘ has been for some years tormented with a
‘ stone in the bladder. He had now (March,
‘ 1790) been for some weeks unable to leave
‘ the house, and in frequent and violent pain.
‘ He took the sal fodæ pills; in one week
‘ his pain was much relieved, and in a short
‘ time he was able to go about as usual. He
‘ still continues much better, and able to
‘ ride gently, but is seldom many days with-
‘ out some pain. He soon became tired of
‘ the pills, and in the whole did not take
‘ them more than a month.’

DR. BEDDOES, *Oxford*.

In a letter dated June 16, 1792, Mr. Jones informs me, that of these patients the third has since died of another disease, and of the fourth, the symptoms have several times recurred, and that with far less severity than formerly. He had recourse to the
F medicine

medicine a second time, ‘ but it produced
 ‘ such violent heat in his stomach, that he
 ‘ could not persevere ; he continues how-
 ‘ ever much better than he used to be,
 ‘ and able to ride about his farm.’ ‘ I
 ‘ have,’ continues my correspondent, ‘ given
 ‘ the pills in several more cases of gravel
 ‘ in the kidneys, and always with success.’
 I have recommended for the last patient
 the solution with cream of tartar, and, if
 that should fail, the impregnated alkaline
 water.

The following letter will be found particularly interesting, as it affords hopes of relieving a very distressing complaint, for which as for so many others, we have no adequate remedy.

‘ *Skiffnal, June, 1792.*

‘ DEAR SIR,

‘ I AM very glad to hear of your intention to publish your formula, with observations on the soda, but can contribute little to your stock of information upon
 ‘ the

‘ the subject. You are already apprised of
‘ my opinion of the great use of vegetable
‘ and fossil alkali in calculous concretions.
‘ No instances having occurred to me,
‘ wherein its effects seemed to deserve very
‘ particular remark, I am unable to offer
‘ more decisive or more favourable evidence
‘ of this medicine, than what springs out of
‘ the general result of my experience. No
‘ opportunity has yet offered to me of try-
‘ ing its power as a solvent of stone in the
‘ bladder ; but in the nephritis calculosa I
‘ have had abundant experience of its effi-
‘ cacy, both in promoting the discharge of
‘ calculi with ease, and often in preventing
‘ their formation altogether. Yet amongst
‘ the many favourable proofs which present
‘ themselves, I must not conceal a few in-
‘ stances, wherein the continued use of this
‘ remedy has, without any assignable cause,
‘ failed to procure relief. The proportion
‘ of these unsuccessful cases is but small,
‘ and as an additional alleviation, I can ad-
‘ duce some proofs of the utility of the
‘ soda

‘ foda pills in relieving biliary concretions,
‘ which I know not if I have mentioned to
‘ you before, and if not, two of them you
‘ may perhaps think deserving the trouble
‘ of a short recital. In both cases the pa-
‘ tients were females, and far advanced in
‘ years. In the first instance, the disease had
‘ existed upwards of fifteen years, and re-
‘ turned pretty regularly at intervals of six
‘ or eight weeks, and sometimes oftener;
‘ the proxyfms occurred with violent pain,
‘ which usually lasted many hours; the skin
‘ became deeply tinged with yellow, and
‘ after a day or two, a flight diarrhœa seem-
‘ ed to relieve her. With some difficulty I
‘ perfuaded her to try the remedy, which
‘ was made into pills with soap, and a few
‘ drops of oleum nucis moschatæ were
‘ added. Upwards of three years have now
‘ elapsed since she had any return of the
‘ symptoms. The other lady had suffered
‘ extremely from successive similar attacks,
‘ and was relieved about eighteen months
‘ ago by the same remedy, but is under the
‘ necessity

‘ necessity of a regular perseverance in the
‘ use of it.

‘ Believe me, Dear Sir,

&c. &c.

‘ W. YONGE.’

Dr. BEDDOES.

Several questions of great difficulty and subtlety may be proposed respecting the operation of alkaline substances in such cases as the preceding. 1. Do they merely produce the expulsion of concremented matter? or do they excite such an action of the uropoietic organs, as tends first to produce, and then to expel this matter? I am disposed to adopt the former supposition; since concretions are often lodged in the passages without exciting any pain, and since I have oftener than once witnessed the discharge of small calculi about the probable period of the beginning operation of the medicine.—Do alkalis act as lithontriptics or otherwise? In several cases, as that of Bramah, they cannot be easily imagined to have any such operation.

operation. The effects are much too sudden; the speedy and considerable discharge of gravel makes it probable that they produce, in the pelvis of the kidney or ureters, some movements, which observation shews to be by no means necessarily connected with an increased secretion of urine. When the patient feels this kind of irritation too sensibly, the dose of the medicine must be reduced. In some instances, solution seems really to have taken place, as in Case 1st, related by Mr. Jones; and many such are to be found on record. Our secretions may, it should seem, be altered, either by altering the action of the secretory organ, or by presenting to it materials different from those which it has been accustomed to work upon. The urine, by whatever cause, is remarkably changed: we are informed on very respectable authority, that the urine first made after a dose of mephitic alkaline water, 'will change turnsole paper to a blue colour, even if it be not taken above a quarter of an hour before the discharge of the

‘ the urine*.’ I wish some other test of alkalis had been used: and still more, that those who have an opportunity of watching the progress of patients under a course of alkaline medicines would ascertain—

1. Whether the blood undergoes any determinate change.—2. Whether the urine becomes habitually alkaline—3. Supposing this to be the case, and the patient to take vegetable alkali, is the excess of alkali in the urine, *vegetable*, or *fossil*, or *volatile*?

Many other equally curious points of investigation would arise in such an enquiry. Of all the secretory organs, the kidneys and the *mammæ* are most certainly and quickly affected by the passions and by food. By studying the analogy that subsists between these organs, and by experiments not difficult to be contrived or executed, it is probable that we may acquire a considerable accession of useful knowledge on a subject little understood.

* Account of the Efficacy, 3d edit. p. 117. Ordinary urine is known to be acid: Mr. Berthollet, the great Parisian chemist, has observed the quantity of this acid to be much increased by certain disorders.

Observations and Conjectures

On the S C U R V Y,

On O B E S I T Y,

On P H T H I S I S P U L M O N A L I S,

AND

On C A T A R R H S.

I N one or two diseases, the patient is apparently directed by a sort of instinct to the means of cure : in all other instances, remedies must have originally been the pure bounty of accident. A few facts, however, would set speculation to work ; and in proportion as the number of substances capable of producing considerable changes upon the living system increased, the data for anlaogical reasoning in medicine were multiplied. It is, in fact, only by seizing those new analogies, which are offered from time to time by discoveries in the physical sciences,

that we can hope to improve the art of medicine. Nor will any one, who will take the pains to comprehend this simple truth, require an apology for attempts to form new combinations of this sort.

Mayow not only discovered several elastic fluids, and the essential properties of the most active of them all, but he aspired to change the whole face of medicine and physiology, by the application of his wonderful discoveries to the appearances of animal nature. Immediately upon the revival of this branch of chemistry, Dr. Macbride attempted to derive the same advantage from it, and the minds of succeeding philosophers have been engaged by similar speculations.

The two annexed papers of Dr. Girtanner afford an extremely ingenious specimen of such speculations. His experiments are several of them happily imagined; that by which he has at once shewn the falsehood of the conclusion which Fontana had drawn from his laborious experiments on poisons,

is conceived in a spirit far more philosophical than that which has directed the greater number of experiments upon animals. His reasonings are an attempt to investigate the laws of organic bodies, by combining the medical opinions of Dr. Brown with Mr. Lavoisier's theory of chemistry. He borrows little from Haller, besides the term *irritability*, and if he has not mentioned the name of Brown, he has made a free use of his doctrines, and often employed his very expressions. He might not, perhaps, in an abstract, think it necessary to point out the source from which so many of his ideas are derived, but in his larger work we have a right to expect that he should do justice to departed genius.

The *Journal de Physique*, from which these papers are taken, is indeed abundantly known to the cultivators of science, but it can by no means be supposed to fall into the hands of the majority of the practitioners of medicine in this island; and I was very willing to enhance the value of this little

publication by annexing to it a train of reflections, in general well worthy of their notice, and calculated to excite their reflections. Mr. Woodhouse, of the Middle Temple, at my request, dedicated some hours of leisure at Oxford to the translation, and his knowledge of the French, and of his own, language, would probably have concealed from the reader that he has not made physiology his peculiar study.

For several years past I had been attempting to discover some part of the effects of oxygen air upon the animal œconomy: it appeared likely that its abundance or deficiency would sensibly affect the health, and that the chemical composition of the fluids and solids of the living body would influence their properties not less than the properties of dead matter, though not perhaps exactly in the same way. In some instances I thought I perceived as much certainty as either could be expected, or as is any where to be found in medical reasonings, and in others there appeared a faint glimmering of probability,

probability, where total darknefs has hitherto prevailed. The ſcurvy (ſea ſcurvy) I have long conſidered as offering an application of the pneumatic chemiſtry, nearly as direct and beautiful as the phænomena of reſpiration ; and it would be eaſy to prove, by the teſtimony of different perſons, that I had long ſuppoſed this diſeaſe to be owing to a gradual abſtraction of oxygene from the whole ſyſtem, juſt as death is produced in drowning, by withholding all at once the ſame ſubſtance from that blood which is to paſs to the poſterior cavities of the heart. The proofs of this theory ſeemed equally ſimple and ſtrong ; the livid colour of the blood, and the large livid ſpots which are ſo often ſpread over the ſurface of the body, left little room to doubt of the abſence of oxygene ; and the recovery of the ſick, by the adminiſtration of acids, and by a vegetable diet, afford a ſort of confirmation ſimilar to that which is derived from chemical ſyntheſis, for no ſubſtances are better calculated than acids at leaſt, to impart oxygene to the ſyſtem ; they contain it in abundance, and they eaſily part with it.

There

There must frequently be observed by those who attend to the effect of evidence upon different minds, a species of intellectual cowardice, which refuses its assent to just evidence, as long as a single difficulty remains, though the facts constituting this difficulty do not oppose, any more than they coincide with, the theory. In hopes of clearing up such difficulties, and applying the hypothesis to the principal symptoms, I from time to time deferred the publication of an opinion, in support of which I had collected various proofs.

In the mean time, the same theory occurred to a physician, whose acquisitions and powers of reflection do him the more honour, as the greater part of his life seems to have been spent amid the hurry and incommodioufness of a seafaring life.* Such an anticipation is very natural in the present

* *Observations on the Scurvy. By Thomas Trotter, M. D.* 1792. In the first edition of this ingenious treatise no such theory is hinted at. The first edition was published in 1786.

period of science, and however mortifying it may be to vanity, it affords the most powerful encouragement to persevere in attempts to ascertain the principles of the most important and most imperfect of all the arts, inasmuch as it proves the possibility of reasoning successfully concerning a disease, which you scarce know but by description.

Dr. Trotter, (from page 125 to 150) states the leading circumstance in this disease, the privation or diminished proportion of oxygene in the blood. He quotes some of the fine experiments of Dr. Goodwyn, and mentions the existence of oxygene in acids, which he justly imagines, they restore to the blood. He does not enter into any other particulars of theory. ‘The *ratio-symptomatum*,’ he modestly observes, ‘is certainly a difficult subject to enter upon. In what manner a diseased state of the blood communicates its influence to the moving powers of the body we are at a loss to explain.’

It may be expected of a theory framed by two persons independently of each other, that the shades should differ, although the outlines be the same. My reflections on the scurvy have been more minute, and, in many respects, my conclusions vary from those of this experienced writer. I shall leave to the reader's consideration the two leading proofs stated above, and so largely expatiated upon by Dr. Trotter, and add some miscellaneous observations, which may tend still farther to establish the theory. I shall also frankly intermix my objections to some of his opinions, cheerfully submitting it to his ample experience to decide between us, in a full expectation that his regard to truth and utility will absorb all personal considerations.

1. In the first place, it is not quite accurate to impute the disease to a deficiency of oxygene in the blood. The deficiency is, doubtless, general to the whole system. The discolouration of those of the solids, of which the colour evidently depends upon

oxygen distributed to them from the blood, bespeaks a deficiency here also. This discoloration is noticed in most of the accounts we have of scurvy. ‘The gums have an unusual livid appearance,’ says Lind (3d edition. p. 100). In one passage the heart is said to be white and putrid, the lungs blackish and putrid (p. 240); in another the muscles are described as mortified (p. 242), and as falling to pieces on being handled. Dr. Blane could not discover, upon dissection, any effusion of blood *, where the livid spots appeared. Hence too it may be concluded, that the hardness and stiffness of the muscles and tendons, a symptom generally observable in scurvy, depend upon the absence of oxygen.

As oxygen is necessary to the contraction of the muscles, it is probable that it is consumed, or, more properly speaking, enters into some new combination, in

* Observations on the Diseases of Seamen, 1789, p. 515.

consequence of which it is eliminated out of the body; for we cannot but suppose that the quantity employed corresponds to the vast quantity imbibed. Hence we may understand the final cause of quickened respiration, during great exertions of the muscles. This seems also to explain an observation which has so frequently been made at sea, that the scurvy makes its appearance after a storm, when the seamen, having undergone violent exercise, have expended a great part of the oxygene of the solids.

2. One of the most pleasing, and, if I conceive it justly, of the strongest possible arguments, may be drawn from some appearances in the thorax after death. Dr. Goodwyn found, that in suffocated animals the left cavities of the heart are full of venous blood. I have had abundant opportunities of verifying the truth of this observation. The death of scorbutic patients frequently seems to ensue, in consequence of the blood being so destitute of oxygene, as to be incapable

capable of stimulating the left auricle and ventricle of the heart : perhaps also the irritability of the muscular fibres of the heart itself may contribute to this effect. The following passages will establish the fact. ‘ The
 ‘ cavities of the heart were quite full of *cor-*
 ‘ *rupted* blood.’ Lind, p. 240. ‘ All those
 ‘ who died suddenly, without any visible
 ‘ cause of their death, had,’ according to
 another account, ‘ the auricles of their
 ‘ heart as big as one’s fist, and full of coa-
 ‘ gulated blood.’ p. 241.

Dr. Lind, from his own dissections, mentions’ that ‘ in patients, whose deaths were
 ‘ unexpected and sudden, and where no
 ‘ effusion of blood could be perceived into
 ‘ any cavity of the body, the heart was
 ‘ commonly much distended with blood ;
 ‘ the auricles and ventricles of both sides
 ‘ were filled, but those on the right in the
 ‘ greatest degree.’ p. 503. This phænomenon would engage Dr. Lind’s attention the more, as he was aware that though the right ventricle after death is generally dis-

tended with blood, ' the left seldom contains any.' p. 503.

This want of stimulating power in the blood, and of irritability in the heart, will perhaps account for the symptoms so often mentioned, anxiety, tightness of the breast, difficulty of breathing.

3. Dr. Trotter considers, ' a deficiency of *recent* vegetable matter *alone*, as the occasional cause of the scurvy.' p. 171, 172. Yet we are certain that the blood, in the first instance, and afterwards the solids, are oxygenated by means of the lungs. They may acquire this principle by means of the stomach; but we have no direct experience of their doing so. It is only an inference from the composition of acids and vegetables, and from their effects in the scurvy. Between the reception of any given food into the stomach, and the oxygenation of the blood by that food, there must intervene unknown processes. It appeared therefore probable to me, that as
seamen

seamen in general breathe an air containing a smaller proportion of oxygene than any other description of persons, the scurvy might often originate from this cause, and may be prevented or cured by guarding against it; for whether we oxygenate the blood by the lungs or the stomach, a disease depending on the want of this principle may, one would suppose, be equally obviated; nor would it be easy in the present state of our knowledge to assign any circumstance that distinguishes the two cases, except the heat supplied by oxygene, when presented in the state of an elastic fluid, though it would be rash to deny that other differences may exist. Captain Cooke's unexampled success in preserving his crews from the scurvy during his two last voyages, seems in great measure owing to his extreme care to keep his ships well aired. On many occasions they were reduced to salt provisions, and much longer out of sight of land, than many other ships, which have been dreadfully afflicted with the scurvy; in his last voyage there
never

never appeared among his crew any symptom of this disorder ; and in his second only one man had it in any considerable degree.

4. It is extremely difficult to find such precise facts as shall amount to an *experimentum crucis*, especially as observation has not yet been guided by this theory. But Dr. Trotter has himself furnished an important observation, from which, if any one were to decide between these two causes of scurvy, want of fresh vegetables, or want of air sufficiently furnished with oxygene, he must, I think, decide without hesitation in favour of the latter ; and here I appeal to Dr. Trotter against himself. In July 1783, a ship, of which he was surgeon, arrived at Cape La How, on the Gold Coast of Africa. In the space of a week above ‘ an hundred prime ‘ slaves, young, stout, and healthy,’ were purchased. The competition however of the purchasers at Anamaboe, whither this ship afterwards sailed, ran so high, that in February, 1784, it had not on board two thirds

thirds of its complement. An indisposition now began to prevail among the slaves, which soon afterwards proved to be the scurvy; and before the arrival of the vessel at Antigua, of near six hundred and fifty, near fifty had died, and about three hundred were tainted, in different degrees, with the scurvy. Before they quitted the coast, seven or eight had died, and between seventy and eighty were ill.

1. Of these slaves, ‘the food consisted of beans, which were brought from England, and rice and Indian corn, which were bought on the coast. These articles were boiled to the consistence of a soft paste, and made as near as possible like the food of the country, by the addition of palm oil, Guinea pepper, and common salt—they were allowed to drink what water they pleased.’ p. 52.

2. They were ‘confined below sixteen hours out of twenty-four, and permitted no exercise when upon deck.’ (ibid.) ‘The
‘rooms,

‘ rooms, where they are secured below, are
‘ from five to six feet in height. They
‘ are stowed *spoonways*, and so closely
‘ locked into one another’s arms, that it is
‘ difficult to move without treading upon
‘ them. The rooms are imperfectly aired
‘ by gratings above, and small scuttles in
‘ the side of the ship, which, of course,
‘ can be of little use at sea. The gratings
‘ are also half covered when it blows hard,
‘ to keep out the salt spray or rain. The
‘ temperature in those rooms, when they
‘ become crowded, was above 96° of Fahrenheit’s scale.—I myself could never
‘ breathe there, unless under the hatchway.
‘ In such situations it may be supposed,
‘ that the sufferings of these creatures are
‘ sometimes dreadful. Air, heated and
‘ rarified to such a degree, and loaded with
‘ animal effluvia, cannot fail of being noxious to life. There were certainly instances where some expired from suffocation, having shewn no previous sign
‘ of indisposition.’ p. 54, 55.

3. With these two facts, let the reader compare the following: ‘ Few of the
 ‘ boys had any scorbutic symptoms ; none
 ‘ of them were shackled ; and by being
 ‘ allowed to run about the deck, and occa-
 ‘ sionally assist in the duty of the ship,
 ‘ their health seemed to be preserved by
 ‘ the exercise. This was also the case
 ‘ with the women, for out of the whole
 ‘ number eight only were affected.’ (p. 63.)
 ‘ During this sickly state of the ship, none
 ‘ of the sailors were in the least tainted
 ‘ with the scurvy. Their diet was the
 ‘ common sea fare ; a little of the victuals
 ‘ prepared for the slaves was generally eat
 ‘ with the salt beef ; *they had it however in*
 ‘ *their power* * to barter some of their pro-
 ‘ visions with the natives for fresh vegeta-
 ‘ bles.’ p. 64.

The following is the inference which the author draws from these facts : ‘ I am extremely unwilling to admit the conta-

* Did they all make use of the opportunity ?

' gious nature of this disease ; and if it
 ' cannot be propagated that way, it is
 ' most likely that the tainted atmosphere
 ' of the slave rooms, which were now full,
 ' so powerfully predisposed those late pur-
 ' chased negroes to scurvy, that the ex-
 ' citing cause was much accelerated in its
 ' operations, by the foul air which they
 ' breathed ; impure exhalations have there-
 ' fore been deservedly mentioned by au-
 ' thors among the *remote* causes of the
 ' scurvy.' p. 68. For my part, I should
 presume that the diet of these unhappy peo-
 ple could have no share whatever in pro-
 ducing the disease ; which delivered so
 many among the men from their present
 and impending calamities.

5. There are other facts which seem to
 shew that too much is attributed by Dr.
 Trotter to *fresh* vegetables. Linnæus in-
 forms us, that the Laplanders are unac-
 quainted with the scurvy ; they feed all
 the winter on the fresh flesh of the rein-
 deer. ' This exemption of the Laplanders
 ' from

‘ from the general distemper of the north,’ says Pringle, ‘ is the more observable, as ‘ they seldom taste vegetables, bread never.’ (Cook’s Voyages, from 1772 to 1775. vol. ii. p. 376.)

Considering fresh meat, or the muscular part of animals, chemically, I see no reason why it should not be efficacious in preventing or curing the scurvy. Oxygene it contains, when *raw*, in a state of loose combination, though probably not in such large proportion as vegetable substances, even such as are not acid. I had noticed in travellers of great respectability passages that confirm this idea. The nations inhabiting the cold and dreary regions on the eastern shores of Asia, and the opposite coasts of America, seem to have learned from experience, that fresh, or at least unsalted fish is a preventive of the scurvy, or a remedy for it. Thus Dr. Pallas (Reise, iii. 47.) describes the Ostiack Tartars of the Oby, as preparing their winter stores altogether

1 2

ther

ther without falt. ‘ They are extremely apt,
 ‘ when disabled by age or infirmities, to be-
 ‘ come scorbutic. In winter they ravenously
 ‘ devour their frozen fish *raw*, a practice
 ‘ which the neighbouring Ruffians imitate,
 ‘ esteeming them a preservative against the
 ‘ scurvy.’ (46.) Mr. Meares (Voyage, Introd.
 p. 30.) speaking of an American tribe, says,
 ‘ She made us sensible that the same dis-
 ‘ order (scurvy) prevailed in her nation;
 ‘ and that whenever the symptoms ap-
 ‘ peared, they removed to the southward,
 ‘ where the climate was more genial, and
 ‘ where plenty of fish was to be obtained,
 ‘ which never failed to prove the means
 ‘ of their recovery.’

The reader will probably agree to consider the frozen as fresh fish. If it were possible to preserve meat on ship board, in this simple manner, one great source of the scurvy would probably be cut off. Cookery combines the oxygene anew; would our sailors eat *raw* animal food?

Dr.

Dr. Lind, though he has full confidence in green vegetables, and affirms that the scurvy never can ‘ become a general, fatal, and destructive calamity,’ where they abound, and the proper method of treatment is known and pursued (p. 541.) ; concludes from a number of comparative trials (p. 538), that certain patients in Haflar hospital in general grew better, notwithstanding they abstained altogether, from vegetables. ‘ This strict abstinence from the fruits of the earth,’ says he, ‘ was continued long enough to convince me, that the disease would often, from various circumstances, take a favourable turn, independent of any diet, medicine, or regimen.’ We have nothing, I presume, to oppose in point of conclusiveness to such experiments made by a physician so intelligent and so experienced in this particular disease.

The following case, for which we are indebted to Dr. Sandifort, Professor of Anatomy at Leyden, is among the most

extraordinary in the records of medicine. Its value however exceeds its singularity. Dr. Goodwyn quotes it in support of his doctrine of respiration. It no less corroborates the foregoing theory. After the local symptoms arising from the conformation of the heart are set aside, there will remain the principal characteristics of scurvy, livid spots or blotches, a bloated countenance, hæmorrhages, excessive lassitude, distressing anxiety, frequent faintings upon slight motion, and a very offensive breath, without an impaired appetite, and with a tendency to salivation. Distinct as the account is, I wish for still greater minuteness, in a firm persuasion that every additional circumstance, by affording a new analogy, would tend still further to shew that the symptoms of scurvy arise from a deficient supply of oxygene to the blood by way of the lungs. For nearly the first year of the life of the infant in question, who was born November 17th, 1764, there was no appearance of disease. The parents were healthy; the child was put out to nurse; he

he grew apace ; ‘ but was scarce a year
‘ old *, when those dreadful symptoms,
‘ which so much harrassed him during the
‘ subsequent portion of his life, manifested
‘ themselves. The livid colour of his
‘ nails and fingers first drew the attention
‘ of the parents. This hue was not con-
‘ stant ; it was not occasioned by any
‘ tightness of dress ; it did not at first ap-
‘ pear of such consequence, as to induce
‘ them to call in a physician, especially
‘ as the child seemed in other respects
‘ healthy, and made such progress as to be
‘ able, by the end of the second year, to
‘ walk alone.

‘ He now began to complain of great
‘ lassitude upon the least exertion. A
‘ catarrh, accompanied with a violent op-
‘ pressive cough came on. The child ob-
‘ stinately refused the medicines prescribed
‘ by the physician, who was now called
‘ in, and the next day a number of spots,

* *Observationes Anatomico-Pathologicæ, Lugd. Batav.*
1777. p. 11, & seq.

‘ which at first were of a red colour, and
‘ then turned livid, were observed upon
‘ the face. The cough continued trouble-
‘ some; motion became exceedingly dis-
‘ agreeable; and after any fatigue, the
‘ face, hands, and feet appeared remark-
‘ ably livid; the tongue and lips were
‘ nearly black, but the natural colour re-
‘ turned, upon remaining quiet some time.
‘ These alternations were visible almost
‘ every day. Meanwhile the child grew
‘ rapidly; the appetite was very good, and
‘ there was no complaint, but of lassitude,
‘ pressure on the top of the head, anxiety,
‘ especially in winter, and such chilliness,
‘ even internal, that in winter he could
‘ not keep himself warm by the fire-side;
‘ nor did the rays of the sun, on the hottest
‘ days, produce their natural effect, much
‘ less occasion any sweating.

‘ March, 1767. Bleeding seemed to
‘ lessen the anxiety and sense of pressure
‘ for a time. The blood was thick and
‘ black, and no crassamentum separated.

‘ Towards the close of this year he had
‘ the small-pox mildly, and without any
‘ aggravation of his anxiety; nor did the
‘ measles and chicken-pox, which he
‘ caught a few months afterwards, produce
‘ any change either for the better or the
‘ worse. The symptoms above mentioned
‘ attacked him the moment he moved,
‘ especially the anxiety, which was accom-
‘ panied with such violent palpitation, that
‘ the strokes of the heart could be seen,
‘ and even heard. Riding which was now
‘ recommended, proved of no service, nor
‘ could he bear it long at a time.

Gaubius, being consulted in 1769, advised cold bathing and rubbing the body, but to no purpose; bleeding, and gentle motion were equally ineffectual; the anxiety, when extreme, was attended with a dry cough. ‘ The breath had an un-
‘ common smell, much resembling that
‘ of an egg opened immediately after
‘ boiling.

‘ This distressing situation continued till
‘ 1774, by which time the anxiety and
‘ violence of the palpitations were much
‘ increased, and faintings, pain, like pres-
‘ sure, in the head, a swelling and pulsation
‘ of the jugular veins, were observed.
‘ Riding being utterly impossible, frequent
‘ airings in a carriage were substituted in
‘ its stead. In May bleeding diminished
‘ the anxiety; and he could now walk
‘ about for an hour, without any great fa-
‘ tigue, and his parents began to entertain
‘ hopes of his recovery. But all the symp-
‘ toms recurred with greater violence in
‘ autumn; he coughed violently, and spit
‘ up, sometimes mucus streaked with blood,
‘ sometimes pure blood. For this he was
‘ twice bled in November, within four
‘ days but the anxiety still continued, and
‘ sometimes rose to such a pitch, as to
‘ threaten immediate death. The child
‘ was sensible that he should not long sur-
‘ vive, he often remarked that his disorder
‘ was quite unknown and incurable, and
‘ that

‘ that no person could conceive what he
‘ felt about the heart. Another bleeding
‘ in December afforded a transitory relief;
‘ but the setting in of winter, a season al-
‘ ways intolerable to him, excited the ut-
‘ most alarm in the parents.’

During the following year, the complaints continued equally distressing. In 1776, ‘ all motion became impossible ; on
‘ the slightest exercise, he would faint,
‘ discharge a great quantity of saliva from
‘ his mouth, and continue blind for a time ;
‘ all attempts to relieve him were vain ;
‘ the anxiety was much increased, especially on laying down ; what had formerly
‘ amused him now became indifferent ; his
‘ face was bloated ; his feet became œdema-
‘ tous; yet his life was protracted in misery to
‘ the 8th of March, 1777, when, upon being
‘ seized with excessive anxiety, he died.’

Dr. Hahn, who attended him during the last year, communicated to Dr. S. the following particulars :

‘ Immediately upon the very first glance, I
‘ recognized the complaint of which I had
‘ heard so much, and which report, as
‘ usual, seemed to have exaggerated : the
‘ child was asthmatical ; on the slightest
‘ motion he breathed with such difficulty
‘ that his face and hands became as livid
‘ as in a strangled person ; sometimes they
‘ looked as if painted blue.

‘ The cause of so severe and so long
‘ continued an asthma was obscure ; nor
‘ did it appear certain when the disease
‘ began. The parents, and the physician
‘ who had hitherto attended him, agreed
‘ in assuring me, that he was born healthy,
‘ and that no signs of complaint in the
‘ chest had occurred during the first year ;
‘ nor was it till the second year that the
‘ blue colour and symptoms of asthma had
‘ been observed ; even then the complaint
‘ was not constant, but became worse and
‘ worse, as the patient advanced in years
‘ and size.

‘ The

‘ The child seemed to me handsome,
‘ well-made, and tall for his years. He had
‘ a constant difficulty of breathing, which
‘ increased in cold weather, and upon motion;
‘ the face was bloated; the eyes
‘ were protuberant, fixed, and betrayed uneasiness.
‘ While the asthmatic fit continued, his face appeared as it usually
‘ does in a person who has been long walking
‘ apace against the wind in winter; the cheeks,
‘ point of the nose, the ears, as well as hands,
‘ fingers, and nails became livid; the lips,
‘ tongue, and inside of the mouth were of a deep purple;
‘ the pulsation of the carotids was visible
‘ at a distance; the pulse at the wrist very
‘ fluctuating.

‘ The child was sensible; his temper
‘ variable; but he was commonly morose
‘ and peevish; during his short intervals of
‘ cheerfulness, you could discover in his
‘ eyes and on his forehead, even while he
‘ smiled, a latent sense of suffering; different
‘ complaints were perpetually recurring,

‘ curring, as head-ach along the sagittal
 ‘ future, ear-ach, pain in the breast and
 ‘ left hypochondriac region, of sickness,
 ‘ pain in the belly, extending as low as the
 ‘ os pubis, chilliness, &c.

‘ At different times his sufferings varied
 ‘ considerably. The following circum-
 ‘ stances I constantly observed :

‘ 1. Great dyspnœa upon motion ; visible
 ‘ pulsations in the neck.

‘ 2. A face too full for the habit of the
 ‘ rest of the body ; during his laborious
 ‘ respiration a livid colour of the coun-
 ‘ tenance, a protuberance, and occasional
 ‘ suffusion of the eyes.

‘ 3. Urine always high-coloured, with-
 ‘ out sediment.

‘ 4. Great costiveness.

‘ 5. Constant chilliness, even though
 ‘ the

‘ the skin felt warm ; this sensation never
‘ left him, except after he had become
‘ quite warm in bed. In winter, though
‘ sitting close by the fire, he complained of
‘ shivering, and in summer he longed for
‘ a large kitchen fire, and desired to bask
‘ in the sun during the hottest part of the
‘ day.

‘ 6. He was sometimes much relieved
‘ by opening medicines and by hæmor-
‘ rhages from his nose, which happened
‘ from time to time.

‘ The tongue was very foul ; the breath
‘ extremely offensive (*fætidissimus*).’

After quoting some cases, not, I think, altogether in point, Dr. S. gives the following account of the dissection. The thorax alone was opened.

‘ The pericardium did not, as usual,
‘ appear surrounded by the lungs, and al-
‘ most inclosed in them ; but a mass was
‘ seen

‘ seen to fill nearly the whole cavity, and
‘ to compress the lungs extremely: this
‘ mass was the pericardium, containing the
‘ heart in a state of great distention, and
‘ very full of blood. It reached from the
‘ diaphragm (which on the right side rose
‘ to the fifth, on the left only to the sixth
‘ rib) to the space between the first and
‘ second rib, and so entirely filled the late-
‘ ral parts of the thorax, that only the an-
‘ terior portion of the lungs on the right
‘ side (viz. the margin of the superior
‘ and middle lobe), and but a very small
‘ portion on the left (viz. towards the
‘ upper and lateral part) could be seen.
‘ Above the pericardium, the superior
‘ cava, with the origin of the subcla-
‘ vian veins, appeared turgid with black
‘ blood.

‘ Upon opening the pericardium, some
‘ water ran out; but not more, indeed not
‘ so much, as is sometimes found in sub-
‘ jects where no dropical symptoms have
‘ preceded death.

‘ The heart, when freed from its sack,
‘ appeared excessively turgid, not however
‘ equably so: both the ventricles were not
‘ distended to the same degree; the right
‘ ventricle, as well as its auricle and sinus,
‘ were much more enlarged, and full of
‘ blood, than the left; all the veins, which
‘ ramify from the coronary veins along the
‘ surface of the heart, were so dilated even
‘ to their extreme branches, that the most
‘ successful injection could not have ren-
‘ dered them more distinct.

‘ The veins arising from the subclavian,
‘ more especially the jugular, were enor-
‘ mously distended with thin black blood;
‘ the superior vena cava, where it is
‘ lodged within the pericardium, did not
‘ much exceed its natural size; the inferior
‘ cava was enlarged; the pulmonary veins
‘ were turgid, but not exceedingly so;
‘ the aorta was enlarged at its origin; the
‘ pulmonary artery was remarkably con-
‘ tracted, from its origin almost to its bi-
‘ furcation; of the arterious duct, or rather
‘ ligament,

‘ ligament, as it would have been at this
‘ age, there was no vestige.

‘ The lungs externally had no morbid
‘ appearance; but they were small, com-
‘ pressed, and not easily dilatable; whence
‘ it appeared that they could not properly
‘ have performed their functions.

‘ The external appearance of the heart
‘ shewed where the source of the mischief
‘ lay. After tying up all the vessels, it
‘ was submitted to further examination.

‘ The right sinus and auricle were first
‘ opened; a large quantity of thin black
‘ blood flowed out. In the foramen ovale
‘ there was an aperture, which would ad-
‘ mit a large probe.

‘ On introducing the finger into the right
‘ ventricle, and turning the point towards
‘ the orifice of the pulmonary artery, where
‘ it usually arises from this ventricle, no
‘ such orifice could be felt, but it easily
‘ slid

‘ flided into another, and that a very large
‘ one. But how great was my astonish-
‘ ment, and that of all the bye-standers,
‘ when it was discovered, that the finger
‘ had passed into the aorta, which, accord-
‘ ing to the ordinary law of nature, has
‘ no communication whatever with the
‘ right ventricle.

‘ This ventricle was divided in the
‘ place opposite to the valve, behind which
‘ the arterious orifice lies, quite down to
‘ the apex. Upon lifting this valve a lit-
‘ tle, the large mouth of the aorta ap-
‘ peared, as also a smooth margin; beyond
‘ which the finger found a way into the
‘ left ventricle of the heart. Upon cutting
‘ the aorta transversely, at a proper dis-
‘ tance from the semilunar valves, the
‘ same margin was seen to divide its orifice
‘ into two parts, the larger communicating
‘ with the right, the smaller with the left
‘ ventricle.

‘ The aorta therefore arose from both
‘ ventricles,

‘ ventricles, and must have received all the
‘ blood from both.

‘ The pulmonary artery having been
‘ cut across above the valves, they ap-
‘ peared very small, almost grown to the
‘ artery, and covered with a granular sub-
‘ stance, resembling a fleshy excrescence,
‘ so that only area enough was left to
‘ suffer a small probe to pass into the ven-
‘ tricle, and even this passed with greater
‘ difficulty from the ventricle into the
‘ artery. Upon opening the orifice longi-
‘ tudinally, we found only two shapeless
‘ valves, partly covered with the same gra-
‘ nular excrescence.

‘ In the mouth of the aorta there were
‘ three valves; in the left ventricle there
‘ was nothing remarkable, except the aper-
‘ ture in the septum, and the thinness of
‘ its substance, which did not exceed, and
‘ indeed scarce equalled, that of the right
‘ ventricle.’

From the several particulars of this wonderful history and dissection, the reader will be able to draw some important conclusions, besides that already pointed out.

1. The comparative capacity and strength of the ventricles in this case confirms Dr. Goodwin's discovery of the inirritability of the left ventricle by venous blood. Here the right ventricle seems to have performed almost all the labour of circulation: hence its enlargement, its equal or superior thickness, the left not having, in consequence of action, outstripped it in this respect; as it does in healthy persons; hence, probably, also, the larger opening of the aorta into the right ventricle. 2. This case removes all doubt as to the necessity of oxygen to the due action of the muscles; a fact which the penetrating genius of Mayow perceived, though he misconceived the mechanism of muscular action, which we do not yet understand. 3. The Dutch physicians seem to have been somewhat at a loss to account for the health enjoyed by the infant during the first year. But the full

full expansion of the lungs is not necessary to life or health in early infancy, during which, for a shorter or longer period in different subjects, part of the blood passes through the foramen ovale and arterious duct, and is therefore not fully oxygenated *. Dr. Sandifort is doubtless right in concluding, that the structure was connate ; no rupture of the heart could have produced it. His information that the child scarce cried, never coughed, and was extremely quiet during the first year, deserves notice. 4. From what has been said above, the peculiar lassitude felt on very high mountains, and described by M. Saussure † from

* Hence, probably, the *sublivid* infantile complexion, which disappears at various ages, just as the remains of the foetal circulation cease, and is perfectly distinct from the occasional jaundice of infants.

† ‘ Les forces musculaires s’épuisent avec une extrême
 ‘ promptitude.—Ce qui distingue & caractérise le genre
 ‘ de fatigue que l’on éprouve à ces grandes hauteurs,
 ‘ c’est un épuisement total, une impuissance absolue
 ‘ de continuer sa marche——On ne feroit pas à la lettre
 ‘ quatre pas de plus, fût-ce pour éviter le danger le
 ‘ plus

from his own sensations, may perhaps be satisfactorily explained. In this situation
fatigue

‘ plus eminent.——Si l’on persiste a faire des efforts, on
‘ est saisi par des palpitations & par des battemens si
‘ rapides & si forts, dans toutes les arteres, que l’on
‘ tomberoit en defaillance ; si on l’augmentoît encore
‘ en continuant de monter.

‘ La seule cessation de mouvement, dans trois ou qua-
‘ tre minutes, semble restaurer si parfaitement les forces,
‘ qu’en se remettant en marche, on est persuadé qu’on
‘ montera tout d’une haleine, jusques à la cime de la
‘ montagne. Or dans la plaine une fatigue aussi grande
‘ ne se dissipe point avec une telle facilité. Mr.
‘ Picquet se trouve toujours saisi d’une espece d’angoisse
‘ (anxiety), d’un leger mal de cœur, & d’un degout
‘ absolu, desqu’il est arrivé a la hauteur, d’environ 1400
‘ toises au dessus de la mer.’ (Saussure Voyages, 4to. I.
482, &c.)

The profound sleep (*assoupissement, sommeil presque
lethargique*) seems exactly the asphyxia arising from an
improper air. The lassitude is only the first degree of
this asphyxia. The anxiety, palpitations, &c. are all
kindred symptoms, and what almost demonstrates that
they are here imputed to the right cause is a subsequent
observation of Mr. Saussure, who tells us, that on the
Col de Geant, at 1763 toises above the level of the sea,
‘ De charbon ne brûloit que d’une maniere languissante,
‘ & a

fatigue very suddenly comes on, and as suddenly goes off on rest. Now in ascending these rugged heights, the muscular exertion must expend a great deal of oxygene, which the rarified atmosphere will supply but scantily. Hence the necessity of suspending the severe exercise to collect a stock of this principle, and hence we see why it is so soon exhausted. The other ac-

‘ & a force d’être animé per le soufflet.’ (*Journ. de Phys. Sept. 1788, p. 209*)

Among the desiderata in medicine, few are, I think, more felt than greater choice in the means of procuring sleep. Opium, and other diffusible stimuli, in some cases, increase the restlessness; and in most cases it would be better to induce sleep by the abstraction of stimuli, than by exhausting the excitability. Upon this principle we could not have a better soporific than an atmosphere with a diminished proportion of oxygene air: ordinary air might be admitted, when the patient was once laid asleep. Mr. Saussure repeatedly mentions the very sound sleep he enjoyed during the nights he spent in the high situation above mentioned. The cold of which people traversing great heights complain, I partly ascribe to the want of oxygene to support animal heat. From the narratives I collect that the sensible cold is greater than the thermometrical.

cidents of respiration, which have been ascribed to a loss of equilibrium between the external and internal air, probably depend upon the same cause. Mr. Plantade, at the age of seventy, suddenly and placidly expired, beside his quadrant, upon the heights of the Pyrenees. Had he been opened, the left cavities of the heart would, I suppose, have been found turgid with blood, and, had he been carefully observed, his countenance would have shewn signs of strangulation. He died, in short, I conceive, nearly as he would have done in the exhausted receiver of an air-pump, with this difference, that the preceding fatigue had consumed the irritability of his muscles. Young animals bear the abstraction of stimuli better than old ones; and there is, probably, a certain mixture of oxygene and azotic airs that would be fatal to old, but not to young persons. The experiments of Mr. Saussure, Pini, and Reboul, concur in shewing, that, independently of its rarefaction, the atmosphere of very elevated mountains contains a far

M

smaller

smaller porportion of oxygene than that of the lower regions, especially than that of the high vallies of the Alps.

7. Therefore the inhabitants of these great elevations, if there were any, ought, according to the theory laid down above, to be peculiarly subject to the scurvy. But as no persons pass their lives in such situations, we cannot expect from this quarter any decisive facts one way or the other. Nevertheless, the only instance I know of a number of persons continuing long in those inhospitable regions, does actually supply a probability in favour of this doctrine. Mr. Condamine, during the operations of the French academicians, upon the summit of Pichincha, was attacked with scorbutic symptoms*.

(*) Un de'entre nous (Mr. D'Arcet says Mr. Condamine is meant) commença a ressentir des affections scorbutiques, les Indiens et les autres domestiques que eurent des tranches violentes ; ils rendirent du sang. (Bouguer, Mem. de l'Acad. R. des Sciences, 1744

The

The advantage to be derived from a better knowledge of the scurvy is, as in other cases, to increase our power of preventing and curing it. And here a thousand projects present themselves.

1. The necessity of keeping ships supplied with fresh air becomes strikingly obvious. And it would be worthy of another Lind to prove the truth of the theory, by supplying his patients with air containing a more than ordinary proportion of oxygene, which, however, to be done safely, must be cautiously attempted.

I was desirous to know whether the temperature of scorbutic patients differed from the common standard, but I find no observation of this sort upon record.

2. We must build our hopes of prevention, and more especially of cure, upon the acids. The efficacy of the native acids of vegetables is so strongly attested by different writers down to the most recent, as Mr.

Drinkwater (*Siege of Gibraltar*) and Dr. Trotter, that if an adequate supply could be provided and preserved, no other means would be requisite for the preservation of soldiers and sailors, whether only liable to the scurvy, or really attacked by it.

A full trial of the mineral acids, as they are called, especially the nitric and vitriolic, ought, I think, to be made. The vitriolic elixir, the worst possible form has never, even in the opinion of Pringle (l. c. 383.) been properly administered. Water, to which a small quantity of the acid should be added, would obviously be the best form. Had these observations been printed before those of Dr. Trotter, I should confidently have said, that the vitriolic acid is decomposed in the stomach and bowels, and that the use of the nitric acid, as appears also from experience to be the case with the oxygenated marine acid, would require great caution, lest in consequence of its easy decomposition it should inflame the stomach. I read with no small surprize in
his

his work (p. 147), that the nitric and sulphuric acids, 'in whatever matter they are exhibited, pass through the body pure and unaltered, as when taken, into the stomach.' He repeats the same assertion (p. 184.) with respect to the sulphuric acid. It is however certain, that almost all animal and vegetable substances decompose these acids; and, in truth, Dr. Trotter has completely misunderstood the table in the *Methode de Nomenclature Chimique*, upon which alone he founds his opinion. In this table he says, 'are to be found those bodies, of which oxygene is a *compound*,' (he means a constituent part) 'arranged according to the degrees of elective attraction. At the top of the column is water, next follows nitric acid, carbonic acid, sulphuric acid, &c. and not till after the tartarous acid come the oxalic, gallic, citric, and malic acids; hence these acids, by being more easily decomposed, or their radicals and the oxygene being confined in weaker degrees of attraction, they are acted upon by the powers of assimilation

' and

‘ and digestion of the human body ; by
‘ which means they become subjected to
‘ the animal process, and form new combinations with our fluids.’ The latter part of this passage seems to consist of mere words ; at least, the ideas are vague ; and it is certain, that the French philosophers have exhibited only a view of their new names in the column in question, and not the comparative attractions of different bases for oxygen, otherwise the acids must have stood in a very different order. My opinion of the decomposition of vitriolic acid in the alimentary canal is founded upon the answer to questions asked with a view to ascertain this very fact. Persons taking vitriolic acid I have found to be very sensible of the smell of sulphur upon different occasions.

Either therefore vitriolic acid ought to be efficacious, or some clear and satisfactory reason ought to be assigned for its want of power. The same observation I think applicable also to vinegar. Some observations
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in Lind seem to favour this expectation, as where he says, that ‘ the relief obtained
‘ by bathing the legs frequently in a day,
‘ with warm vinegar, is both *quick and sur-*
‘ *prising.*’ Did he make a comparative experiment with warm water ?

If herpetic eruptions, * as there is some reason to suppose, depend upon an abstraction of oxygene, less than that which occasions the scurvy, or rather perhaps upon a local deficiency of this principle, one may presume, that vitriolic acid, the best remedy we possess for the former, will not be inefficacious in the latter complaint. And although I am not led, by the improper application of a common name to confound two diseases clearly distinguished by their symptoms, yet it may, perhaps, without absurdity, be proposed as a query, whether

* The only circumstance I cannot reconcile to this supposition is the efficacy of cantharides in these eruptions. The efficacy of certain metallic oxids, applied externally, favours it extremely.

they

they do not approach each other by a number of intermediate gradations. To illustrate my meaning, as well as to countenance my question, I shall produce an example of a disease, which may, I think, be termed the *scorbutic leprosy, or herpes*; between which diseases again there seems to me no certain boundary. Dr. Pallas observed this scorbutic leprosy in more than one place in the Russian dominions. It makes a much slower progress than the scurvy. Those whom it affects are not sensible, during the first and second year, of any considerable pain or weakness. The countenance appears livid, as if they were strangled; livid spots, little elevated, and giving no pain, are seen in different parts of the body; an herpetic eruption breaks out on the chin; the spots gradually enlarge, and violent pains are felt in the limbs; the herpetic eruption at last overspreads almost the whole of the body, and to the bare, dark red spots succeeds a scurf, consisting of long scales, among which ulcers

frequently break out. Sometimes the scurf falls off, and the skin takes on a natural appearance. If a person happens to wound or bruise any affected part, foul, spreading ulcers appear, and often eat down to the bone. The fingers are particularly liable to such ulcers, and they commonly fall off joint by joint. This loathsome distemper except in its very last stage, does not produce much emaciation or debility. The animal functions go on well. (Pallas, Reise, 302, 303.) The Tartars hold this, which they call, from the complexion of the patients, the *black disease*, to be infectious; yet the physician, who describes it, saw a family, where the elder brother was first seized, the younger three years afterwards, the mother a year after him, while the wives of the two brothers, who had constantly lived with their husbands, continued perfectly free. This observation will perhaps appear decisive against the opinion of these poor people, especially when we reflect how rapidly the logic of terror, among the ignorant in particular, hastens to conclude,

that such and such disorders are catching. Dr. Pallas mentions a particular, in which this disorder of the Cossacks differs from the leprosy; the affinity however is striking.

These Tartars are utter strangers to agriculture. They inhabit a country, where the soil is impregnated with salt, and abounding in salt lakes. Their diet is fish, often salted when it is more than half putrid. About the fort *Jaiskoi Gorodoc* alone, watery fruits, as melons, and the *most indispensable kinds of garden herbs*, are cultivated, but not by the Cossacks, who must buy, if they will have them, and who, therefore, I suppose, supply themselves, but scantily.

3. SOOINS, an acidulous preparation of oatmeal, by which, as Pringle informs us (l. c. p. 382.), one of captain Cook's most intelligent friends cured his scorbutic sick on board, deserves more attention than it has obtained. It is prepared by pouring
hot

hot water upon oatmeal, and suffering it to stand till it has become sourish. The decanted water is to be boiled down to the consistence of a jelly. The efficacy of this fermented acid affords a presumption in favour of vinegar ; and it may, I suppose, be generally procured on ship board.

4. Nitre seems to deserve attention in the cure of scurvy. It is doubtless decomposed in the *primæ viæ*, and capable of supplying much oxygene. These fatal consequences, which have sometimes followed excessive doses of nitre, have been, I imagine, owing to an inflammation of the stomach, occasioned by the oxygene ; and it is, probably, upon the direct or indirect action of oxygene, that the danger of exposing wounds and certain parts of the body depends.

5. Sweet wort, or the *extract of malt*, I conceive, from its chemical composition, to have some, though inferior, antiscorbutic virtues. And the great authority of captain

Cook, who calls it one of the *best antiscorbutics yet found out*, of Mr. Patten his surgeon, and others, strengthens the opinion which chemistry suggests. It is true, as Dr. Trotter observes, that Macbride's theory is indefensible; it is true also, that had there been nothing erroneous in his theory, wort, the unfermented and unfermenting extract of malt, could have had no efficacy in scurvy; neither has it been of late pretended that wort will of itself completely cure the scurvy; but from the considerable proportion of oxygene contained in sugar, it is probable that it will retard the approach, and check the ravages of the disease. Dr. Trotter, I conceive, has much too lightly pronounced, that 'the simple wort passes easily through the body, and that it undergoes no decomposition in the stomach.' And, admitting this, does he suppose that none is absorbed? that it transmitted no oxygene to the system of those hundred and thirty scorbutic patients, who took it for fourteen days, without a single instance 'of sickness, gripes, or purging,' occurring (Lind, p. 539,)

p. 539,) and who, though they were not cured by the wort, appear, as one may fairly conclude from Lind's expressions, to have grown better while they were taking it? It would, I think, be rash and unjustifiable in the highest degree to lay aside a preparation, at once cheap, easy, and palatable, and which appears to possess, in whatever light it is considered, powers of prevention by no means despicable. I am inclined to think much the same of four krout, notwithstanding Dr. Trotter's objections.—The relation between a diet of salted provisions and the scurvy, it is not easy to determine with precision. Such a diet is certainly not necessary to the production of scurvy; the facts already enumerated afford abundant evidence of the falsehood of this hypothesis, and it has, of late, been generally abandoned. Nevertheless, salted meat may be capable of giving rise to the disease, either by itself destroying loose combination in which oxygene seems to be held by hydrogen and azote in the recent fibrous and cellular substance of organic beings, or by suffering

suffering a combination to be destroyed, in which perhaps oxygene is not a great deal more firmly held than by the azote of the atmosphere.

The diet, therefore, of besieged garrisons will, perhaps, account for the production of the scurvy, though the soldiers may not breathe the purest air in their barracks. Prisoners, especially those taken in war, are frequently attacked by this disease; and if we recollect how considerable their numbers have sometimes been, and how close their confinement, we shall be at no loss to understand why they should be visited by this dreadful calamity.

One observation on the means of prevention and cure I will add before I quit the subject of scurvy. Of all the substances that can at once be cheaply procured and long preserved, the concrete acid of tartar seems by far the most promising; it is very grateful, and comes near to the citric acid.

THIS

THIS theory of scurvy seems, with some modification, capable of being applied to the accumulation, or, more properly speaking, the secretion of fat in excess. The oily parts of animals, I believe, principally differ from the rest of the heterogeneous mass, commonly called flesh, in a single circumstance; they appear to contain a smaller proportion of oxygene. The actual convertibility of the softer solids into a substance like spermaceti, is a clear proof of the affinity between muscle, cellular substance, and fat, as to their composition; add or take away a little of one or more of their constituent parts, and the alteration is effected. The conversion, as one may, I think, infer from the observations made in the burying ground of the Holy Innocents at Paris *, depends on some very slight cause.

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* In the large *common graves*, where several hundred bodies lie together, all the soft parts were wonderfully

The distillation of organic matter, violent as that operation is, favours the supposition.

fully changed; in some subjects, where the process of conversion was not finished, a few of the muscular fibres were visible; in the rest, all the muscles, even the heart, the membranes, tendons, and viscera had disappeared; the brain was also changed. The substance, into which all was alike changed, was soft, ductile, greyish white, much resembling new cheese. It frothed extremely with water, and proved to be an ammoniacal soap, from which acids separated a concrete oil, which when dried slowly, was like spermaceti, having a lamellated, crystalline texture; it was much more soluble in alcohol than spermaceti is, and it readily dissolved in cold ammoniac. As it bore some resemblance to wax, Mr. Fourcroy proposes to call it *matière adipo-circuse*.—The soap or *gras* is said, however, not to constitute above $\frac{1}{10}$ or $\frac{1}{12}$ of the body. The rest evaporates in the form of water, carbonic acid air, and perhaps some unknown elastic fluids. (Annal. de Chimie, v. 154. viii. 17.)

Mr. Thouret appears to entertain some doubt as to the conversion. He says they appear to be converted, but imagines the conversion may not be real. He seems to be perplexed by some indistinct hypothesis lurking in his mind, and perhaps is not aware that conversion can only mean a change from the loss or accession of some constituent part. Except the bones, the colouring matter of the bile, of the bronchial glands, the pig-

sition. The water and carbonic acid, having appropriated the oxygene to themselves, the oils, ammoniac, and carbone appear to be formed from the residuary azote and carbone. From Mr. Fourcroy's experiments, as well as from the process of distillation, it looks as if the production of animal oil and ammoniac were operations nearly allied. One would not assuredly reason with much confidence from distillation to secretion, especially as the distilled

mentum nigrum, ' and, perhaps, the *proper* substance of
 ' the muscles, this transformation has entirely subdued
 ' all the other parts; the skin, adipose substance, membranes, muscles, viscera, cartilages, glands, tendons,
 ' aponeuroses, and even the fluids.' Again—' though
 ' the transformation seems to have taken place in the
 ' muscles, it has probably established itself in them by
 ' means of the lymphatic and unctuous juices.' (J. de Physique, April 1791, p. 256—7.) Mr. Halle, in treating the fibrous parts of animals with nitric acid (ibid. Mai, p. 338—9.) obtained an oily concrete, similar to spermaceti, and unalterable by nitric acid. If, as Mr. T. seems to argue, this were really the base of the animal fibre, it might easily happen that during a certain state of the system it should be changed into fat instead of muscular fibre.

differ in appearance from the secreted oils, and doubtless also, somewhat in the proportion of their constituent parts. But is it unlikely that, if the blood itself should ever contain but little oxygene, a substance containing a smaller proportion of oxygene than other animal substances, should be formed? and if muscle, membrane, tendon be changed into fat after death, why may not the powers of life, instead of muscular atoms, occasionally combine into fat those elements, which, under other circumstances would have gone towards the formation of fibres or laminæ? In dissection we avoid fat subjects, when we desire to shew the muscles distinctly. Is it that there is less muscle as there is more fat? or are the fibres, the mass of muscle remaining the same, only rendered more indistinct? Every new hypothesis, if it be but thought worth the trouble of refuting, adds to our stock of well-ascertained facts. Till it be certainly decided, whether, as I suspect, the fat encreases at the expence of the muscle, let us try if we cannot discover, in the living system itself, some obvious appearances,

pearances, indicating a tendency to form fat, whenever there is a deficiency of oxygene to a certain degree.

1. Among the Africans, of whose sufferings on board the slave ship Dr. Trotter has given so particular and affecting an history, corpulence seems to have been, as it were the first stage of scurvy. ‘When a negro was becoming rapidly fat,’ says he, ‘it was no difficult matter to determine how soon he would be seized with the scurvy;’ so that corpulence seems here to have been the harbinger of the scurvy. Writers have been particular in noticing that this disease seldom or never produces emaciation. Dr. Trotter, upon whose information we may place full reliance, tells us, that having purposely enquired among his medical acquaintance in the navy, he did not find one who considered the wasting of the flesh or absorption of fat as a symptom ‘congenial to scurvy.’ He immediately subjoins some observations of his own, that clearly indicate a connection between

the scurvy and obesity (p. 98, 99.) ‘ In a corpulent state of the body,’ he says, ‘ the most hideous features of the disease are expressed; such are the bloated looks and countenance, &c.’ In a mess of midshipmen, who lived altogether on the ship’s fare, the only one he ever saw affected with the scurvy was ‘ a young man remarkably corpulent.’ From the whole of his observations it appears clearly that obesity pre-disposed his patients to scurvy, or rather was to them what cachexy is to dropsy.

2. The emaciation produced by acids, which is excessive, where they are taken to excess, is a fact of which it is only necessary to remind the reader. In cyder countries the people are habitually leaner than where beer is the common liquor.

3. That inactivity which stands to obesity in the relation both of cause and effect, generally prevents fat persons from attempting to reduce themselves; and though they may make a few struggles at first, they finally submit

submit to the incumbrance. They seldom consider themselves as in a state of disease, and a prejudice prevails, that corpulence cannot be cured without danger. Hence we hear but little of the medical treatment of obesity. The authentic facts, however, which we possess, shew that it may be successfully treated in the same manner as the scurvy. This analogy is particularly striking in two cases of unwieldy corpulence, which Fothergill removed by a strict vegetable diet. Mr. Wood, whose case is so well known from his own account published in the Medical Transactions, is another instance of the same kind.

4. Short-winded persons are very often corpulent, and even many asthmatics. Wherever the livid colour * of the countenance

* I mean an occasional or prevailing hue, not a permanently fixed stain, such as the red nose and cheeks of persons poisoned by fermented liquors exhibit. This deformity I suspect to arise from inflamed lymphatics, which, by continued excess in drinking, become indurated. When a red nose alternates, as it often does, with pain in the liver, are the absorbents only or the whole

nance indicates a deficiency of oxygene, there you will seldom fail to observe a full habit. Here I shall probably be told that I put the effect for the cause, since dyspnœa is the consequence of the accumulation of fat. Instances may also be produced of an emaciated habit of body, attended by difficulty of respiration.

In answer to the first objection one would not assert any thing positive; it is a subject on which it is not easy to make decisive observations. It only appears to me, from my remarks on corpulent habits, that dyspnœa and obesity favour each other, that I think it evident also that every different species of dyspnœa, though in appearance whole organized substance alike affected? My reason for suspecting the lymphatics to be principally affected is not only because the career of gross debauchery so frequently terminates in dropsy, but because I have been able to feel cords like scirrhus absorbents in various parts of the bodies of persons destroyed by alcohol. Had so much induration been owing to scirrhusity, lancinating pains must have been felt, which I have been assured by the patients was not the case: nor was there any cause whatever besides these cords for suspecting the presence of scirrhus or the approach to cancer.

equally

equally distressing to the patient, does not equally prevent the action of the air and the blood on each other.

It is not so difficult to elude the force of the other objection. The living body is doubtless subject to the influence of counteracting causes, of which that which tends to diminish the fat may prevail over its antagonist. Fat may be secreted without being accumulated. In a perfect theory, these counteracting causes, I am sensible, ought to be specified, and their power estimated; but we are so far from having attained such precision in the knowledge of any one of those various chemical operations that are comprehended under the improper term *secretion*, that it is making some advance to indicate a probable general cause, even if we are obliged to leave its action to be accurately determined hereafter. I object to the term *secretion*, taken in its proper sense, because many circumstances concur to render it probable, that only the elements of *secreted* substances exist in the blood, and that combination is the office of the

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the secretory organs. No one now, I suppose, will maintain that they are mere filters.

5. The mechanical effect of exercise has never yet been fully appreciated. I have no thought of disturbing the absorbents in the possession of their claims; their action is, I doubt not, increased by exercise. But may not exercise also, by introducing more oxygene into the system*, and by diffusing it more widely, check the formation of a substance containing little oxygene, while the fat, in common with the other fluids and solids, is absorbed? In the course of life there are two periods during which the fat is apt to accumulate; the first is

* A very delicate observation made by Mayow is well deserving of notice in estimating the effect of exercise. A dog that was panting and breathing deeply, on receiving arterial blood into one of his veins, instantly began to breathe so calmly that his respiration was scarcely sensible. The animal received from an unusual source the substance which is probably expended in muscular action; it was therefore no longer necessary to inhale it rapidly.

that of infancy, during which not inaction merely, but, if I am not deceived, the state of the blood also, favours the formation in excess as well as the accumulation of fat. The lungs are not probably fully expanded for a considerable time after birth ; and whatever quantity of blood goes to the aorta by the arterious duct, or to the left auricle by the foramen ovale, which frequently continues open for a considerable time, must so far prevent the oxygenation of the system*.

The commencement of the second is fixed by Haller at forty years of age. At this period of life an indolent disposition is coming on ; neither curiosity nor the other passions any longer agitate us with equal force. At this period I conjecture that there is also a deficiency of oxygene in the system ; and the conjecture will receive

* *Cæcum ductum arteriosum* reperi aliquando post paucos menses, alias serius, ut secundo anno absoluto tubulus pervius fuerit, *Haller*. III. 161. celerius adeo quam foramen ovale clauditur, ib. 162.

some confirmation from the papers of Dr Girtanner; whether it be that the stomach and lungs are so altered as not to imbibe it in the usual quantity, or the other constituent parts of the solids and fluids are so altered in their proportion as to have lost somewhat of their attraction for oxygene, or upon whatever else it may depend. The lessened vigour of the whole system, the diminished irritability of the muscular fibre, the weariness that now so much sooner follows exertion, indicate this, and the rigidity of the tendons, fibres, and laminæ may, in advanced age, be owing to a permanent, as, in scurvy to a temporary, deficiency of the same principle. If this supposition were just, might not some means be discovered to protract the period of youth and vigour indefinitely.—Whether true or false, and even though we should never be able to restore new excitability to the system, there can be no doubt of the immediate practicability of prolonging life considerably, and, what is much more desirable, of maintaining a firmer state of health, by a proper management

management of the excitability during the periods of infancy and youth.

6. It is generally observed that persons who indulge much in sleep are apt to grow fat. From the well-known infrequency of respiration during sleep, a smaller quantity of air must be taken into the lungs than while we are awake. The necessary consequence and its application are obvious. The zoologists supply observations to the same purpose. Thus, *Mures montani hieme pinguissimi fiunt, etiam absque cibo**; *idemque de ursis est notissimum †*; *ut recte pinguescant gallinæ, plurimum ut dormiant necesse*

* Haller Elem. Physiol. I. 39. For this fact he quotes Ray's *Wisdom of God*, but, as I remember, erroneously. Ray, if I do not mistake, says, that the marmot, the mus montanus here understood, is fat in autumn, and comes out lean in spring, just the contrary of what Haller makes him say. I do not therefore build upon what he asserts of the marmot or the bear: the other facts are to my purpose.

† Aristot. Hist. Anim. VI. 36. Hillerstroem Iamtelands Diur. fang.

est *, *eoque scopo ipsum lolium in cibum miscetur* †.

Vegetables doubtless decompound water; it appears almost certain that they must combine the hydrogen with azote absorbed from the atmosphere, to which a certain portion of oxygen is added. How then comes it to pass that these elements are not at times so combined as to produce an excess of oil and a sort of obesity in the vegetables? some such modification of their functions does really take place. In proportion as they are more exposed to heat and light, they seem to form a larger quantity of oils and resins, as well as of saccharine matter, which is nearly allied in its composition to the two former substances. Wherever they are able to decompound moist water, there also they will probably absorb moist azotic air. Must not these two functions go on most

* Reaumur, *Art de faire eclorre des oisieux domestiques*, p. 2. id. 2. p. 392.

† Id. p. 393.

vigorously in those plants, which in the same soil and climate form most oil? We know besides, that vegetables are capable of forming oils, either exactly the same as those of animals, or very nearly resembling them. Thus we have the suet of the *croton sebiferum*, the butter of the *phœnix dactylifera* and of the *butyrum cacao*. When from a more intimate acquaintance with them, we shall be better able to apply the laws of organic bodies to the accommodation as well as the preservation of life, may we not, by regulating the vegetable functions, teach our woods and hedges to supply us with butter and tallow? Thus our pastures and meadows, the most fertile spots in every country, would, many of them, be gained to the cultivation of corn, the immediate food of MAN. And how many millions of inhabitants more might BRITAIN maintain, if we could feed upon the immediate produce of the soil? how many tons of vegetable food are condensed into every fat ox?

WHEN

WHEN the medical practitioner finds himself deserted, in every emergency, by the hypotheses of the schools, he is too apt to attach himself to a blind routine, and to conclude that attempts to explain the capricious phænomena of diseases, for so they must appear to him, will be hereafter as fruitless as they have heretofore been. The next step is to decry the value of that which he neither possesses nor hopes to attain; nor will he tolerate in others attempts to discover the laws of animal nature, of the existence of which it is yet impossible to doubt. Perseverance in the observation and comparison of phænomena, which have hitherto been observed and compared in vain, is the privilege of a few superior minds. Examples of ill success in these difficult investigations do not overpower their firm persuasion of the existence of invariable laws, and of the possibility of a complete theory of diseases. I feel not much reverence for those who
pique

pique themselves upon pure experience.]
There are few diseases in which we have any fixed rule of practice ; and our specifics are so few, and so easily applied, that this part of medicine may be acquired without difficulty or loss of time. In most instances a theoretical deliberation of some sort must precede prescription, and here the discrimination of persons habituated to speculation will have the superiority of skill over chance, and their fertility of resources will appear to peculiar advantage.

In the consumption of the lungs, as indeed in too many other diseases, a conjecture may be offered with the less diffidence, since experience cannot here set up the slightest pretension to overawe speculation. This melancholy truth will, I hope, propitiate those whose displeasure might otherwise arise against an attempt to disturb medicine by the introduction of new ideas. I beg leave also to assure persons little acquainted with the recent progress of science, that I do not employ the French chemical nomenclature

clature from affectation ; but as the French has prevailed over the old theory of chemistry, so I expect that the terms sanctioned by the founders of that theory, however uncouth they may sound at present, will finally establish themselves, partly as being more convenient, partly as being the language in which the most eminent philosophers of Europe communicate their discoveries. Persons acquainted with the substances they design will find no obscurity in the terms, and for others it is indifferent what terms are used.

Not much seems to be gained by ranking the phthical tumours and ulcers of the lungs among scrophulous complaints. We have no very successful method of treating scrophulous sores, wherever situated ; neither have we any tolerably clear idea of the nature of the disease. Not to mention that very different ailments are comprehended under a term so conveniently vague. I see no hopes therefore of transferring any useful ideas from the external appearance of

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scrophula

scrophula to the internal form of the disease, if they should be essentially the same, of which I am by no means satisfied. We see, it is true, strumous persons attacked by consumption; but we may observe many others attacked by it, who have had no glandular swellings, or other marks of scrophula. However this may be, the only circumstance in phthisis, from which, in our present state of ignorance, we can hope to reason to any purpose, has always appeared to me to be the *occasional* effect at least of pregnancy in suspending the progress of phthisis; for if we could once discover how pregnancy produces this singular effect, we might be led to discover also a method of superinducing and prolonging the same change of the system at pleasure.

I had repeatedly attempted to proceed through the obscurity by the help of this clue, but in vain. I have lately had a very favourable opportunity of observing this effect of pregnancy, but could fix on no plau-

fible supposition, with which I might compare the phænomena. At last, when it was too late, the disease having returned and destroyed the patient, the following supposition occurred: The foetus has its blood oxygenated by the blood of the mother through the placenta. During pregnancy there seems to be no provision for the reception of an unusual quantity of oxygene. On the contrary, in consequence of the impeded action of the diaphragm, less and less should be continually taken in by the lungs. If therefore a somewhat diminished proportion of oxygene be the effect of pregnancy, may not this be the way in which it arrests the progress of phthisis; and if so, is there not an excess of oxygene in the system of consumptive persons? and may we not, by pursuing this idea, discover a cure for this fatal disorder?

I am not able to prove the existence of this *phlogisticated* or *putrescent* state of the system, as it would formerly have been called, during pregnancy. I must therefore
appeal

appeal to the present knowledge or future observation of accoucheurs to determine whether it manifests itself in the colour of the blood, bleeding of the gums, dark coloured spots, vibices, or any other scorbutic symptoms. The state of the blood in pregnancy is certainly peculiar; this the writers on midwifery declare in very pointed terms; but I have not found its sensible qualities so well defined as might be wished. Thus we read that ‘the blood of pregnant women is always found to have what is called a fizy appearance, though of a peculiar kind, and evidently very different from that which is observed in cases of inflammation *.’ I know not whether the spots or blotches mentioned by the same author†, or the red spots ‡ mentioned by Mauriceau as covering the legs and thighs, or the dark colour of the abdomen and inferior extremities described by Camper in his discourse on the colour of the negroes, as often seen

* Denman, Introd. to Midwifery, p. 248.

† p. 266.

‡ p. 72.

in pregnant women, which Le Cat had also observed, are scorbutic blotches; and if so, whether they proceed from a constitution habitual during gestation, and in these instances more strongly marked than usual. This constitution, if it exists at all, requires nicer attention than has probably been paid to it, both because it is not likely in general to recede very far from the healthy state of the system, and because some phænomena are seldom well observed, till they are observed with reference to an hypothesis. The natural effect of an habitually straitened respiration will have more weight with the skilful and reflecting reader than this lame and defective evidence. The following coincidence he will perhaps think remarkable. Pregnant women agree with scorbutic patients in that strong instinctive appetite for vegetables, and it appears as if this diet was the most suitable to them. ‘Pregnant women,’ says Dr. Denman, ‘have generally a dislike to animal food of every kind, and under every form—on the contrary, they prefer vegetables, fruit and every thing cooling,

‘cooling, which they eat and drink with
 ‘avidity, and in which they indulge with-
 ‘out prejudice *.’

But is the inference from the supposed deficiency of oxygene in pregnant women confirmed by appearances about phthifical patients? Do they shew any signs of a redundancy of this principle?

The clear, bright, and florid hue of the flushed hectic countenance, so diametrically opposite to the scorbutic complexion, affords some presumption of a state of the blood, equally receding, but in an opposite direction, from the standard of health. The countenance of persons flushed by exercise or food widely differs from that vermilion bloom, which is mistaken by the uninstructed for the sign of health, though it is the harbinger or attendant of an incurable disease †. On holding the hand of a consumptive

* *Introduct. to Midwifery*, p. 249.

† The clearness of skin in persons ill of phthisis, or
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sumptive person against the light, the semi-transparency of the margin of the fingers and joints is, I think, evidently of a more vivid carnation in consumptive patients. A striking difference would probably be perceptible between the hand of a phthical and scorbutic patient. In the former, the appearance may, indeed, be imputed to emaciation; but there are similar appearances which cannot by any means be imputed to emaciation, nor, in my opinion, to any cause except that assigned above. ‘During the fever,’ says a late attentive writer, who did not here look through the coloured medium of hypothesis, ‘the cheeks appear as if painted with a circumscribed spot of *pure florid red*; the lips and the tubercles in

pre-disposed to it, is a general observation. *Qu.* When it attacks persons who have herpetic eruptions, do those eruptions cease?—Is not the chilliness of scorbutic patients, and of the *blue boy*, owing to a want of oxygene? this chilliness seems perfectly distinct from febrile horror and rigor.—And is not that *sense* of heat, which so much distresses phthical patients, owing to *actual* heat, produced by the excess of oxygene?

‘ the

‘ the canthus of the eyes are also redder
‘ than when in health *.’ Again, towards
the termination of the disease, ‘ the tongue
‘ appears clean, and, with the fauces, is of a
‘ *bright red*†.’ Here I request every reader
acquainted with the modern doctrine of
respiration (otherwise he is not a proper
judge) to pause a moment. It will, I ima-
gine, readily be allowed that if the blood-
vessels were filled with more florid blood
than usual, this identical bright redness
would be seen. The cause, then, is ade-
quate to the effect : the change, too, is not,
as in the loose analogies of the humoral
pathology, transported from some remote part
of inanimate nature, and at random im-
puted to animals. If the various evidence
adduced in the case of scurvy should not
prove satisfactory, yet the most determined
sceptic will not hesitate to admit in asthma
a deficiency of oxygen and a corresponding
change of colour. Why then will he not
infer from an opposite change of colour an

* Reid’s Essay on Phthisis Pulm. ed. 2nd, p. 13.

† Ibid. p. 19.

opposite condition of the blood? If in the *puer cæruleatus* of Dr. Sandifort, the tongue and fauces were deep blue or purple, because the lungs did not admit oxygene enough, will it not be granted that a bright red indicates the reception of too much?

‘ But then the blood itself ought, according to your supposition, to be brighter than ordinary;’ the arterial blood certainly; and a comparison of the arterial blood of phthical and healthy persons would be so interesting on this very account, that I wish it were practicable. But if we consider the whole action of oxygene upon the blood, we shall not perhaps expect the difference to be sensible on a comparison of the venous blood; for oxygene sometimes darkens and sometimes enlivens the venous blood; not, however, indiscriminately, but under determinate circumstances. When blood is exposed to oxygene air, it first becomes florid, and afterwards black. Mr. Fourcroy and Mr. Hassenfratz * have ascertained this

* Annales de Chimie, t. IX.

fact in a very careful manner; and I have had several opportunities of remarking the same change. The oxygene, which at first is combined with the whole mass of the blood, or the red globules at least, after some time is attracted by the hydrogene alone or united with azote, and forms accordingly water or carbonic acid. If oxygene be added in a large quantity at once, or loosely combined, the blood is never brightened, but turns immediately black, as when oxygenated marine acid is added to blood.

Probably the solids, during circulation, more than divide with the blood its loosely attached oxygene; if they have a superior attraction, they will, as some of the constituent parts of the blood itself do upon standing, take the whole and leave the blood dark-coloured.

Hence then it appears possible enough that an unusual quantity of oxygene being thrown in through the lungs, the solids might attract it and be consumed themselves,

selves, or prepared for absorption in some way not understood, while the venous blood returns to the lungs of its ordinary colour; and, were its colour changed, the difference in the shade might not be obvious; besides, some of our chemical discoveries are so recent, that time has been wanting to apply them to improve medicine extensively; so that satisfactory comparative remarks on the colour of the venous blood in consumptive patients are not to be expected ready made. We seldom perceive much more obvious phænomena than this probably can be, till we are prompted to look for them.

From Willis to Forthergill, and from Forthergill downwards, scarce any real observation occurs upon the state of the blood. One tells us that it is polluted, another that it is contaminated, a third that it is acrid, a fourth that it is putrescent *, without ever

 recollecting

* Left it should be suspected that *putrescent* is here a sound significant, it should be observed that ‘when we examine the blood drawn from patients in every period of consumption, so far from any appearance
 ‘ of

recollecting that to employ terms expressive of phænomena, such as the senses may recognize and to reason upon such phænomena alone, are indispensable conditions in philosophizing.

Finding such little satisfaction in the writers whom it was in my power to consult, I applied to a gentleman whose opportuni-

‘ of dissolution in its contents, the reverse is constantly
 ‘ found; a *thick buffy size* and *firm crassamentum*.
 ‘ Nay, so inconsistent are the favourers of this doctrine’
 (i. e. of the putrefaction of the fluids and solids in certain diseases) ‘ that the *size*, and the degree of cohesion
 ‘ in the blood, has always been esteemed an indication that the operation ought to be repeated, and
 ‘ much blood has been unnecessarily shed accordingly.
 ‘ Nor in its progress do we observe any symptoms
 ‘ similar to those found in diseases usually termed putrid; *no petechiæ, vibices, fordes about the teeth, or blood*
 ‘ *issuing from the gums, and other parts of the body.*’ Reid, p. 71—2. Dr. — of Edinburgh, whose experience in blood-letting is immense, used to teach that in phthisis there is an unconquerable morbid tendency in the system to generate blood.—The firm crassamentum, according to Dr. Gritanner, is a sure indication of excess of oxygene.—Little disposition to coagulate in scorbutic blood—the same in the *blue boy’s* blood: a striking coincidence!—opposite state in phthisis not less striking.

ties of inspecting the blood of phthifical patients have been ample, and whose fine experiments on frigorific mixtures attest his talents for observation, and will dispose the public to confide in his accuracy. I asked him simply, without any previous communication, whether he had noticed any particular appearance in the blood of phthifical patients. ‘Yes,’ he replied, ‘and that so constantly, that I believe I could generally distinguish blood taken from such patients, especially where the disorder is confirmed.’—On the following written answer, he observed that colours are not easily described, and, perhaps, his terms might not be the best chosen: *florid* and *purple* seem indeed not well to agree.

DEAR SIR,

IN answer to your question whether I recollect to have observed any particular appearance in the blood from phthifical patients, I can inform you that I have always been as it were involuntarily, and without any particular design of attending

to

to it, struck with its thin consistence, and its florid purplish colour.—Upon since putting the same question to a foreign gentleman, who has paid particular attention to its appearance, and even made experiments upon it, he replied, that it differed from other blood in being thinner, more florid, and having a purplish hue.

&c. &c.

Oxford, }
July 25th, 1792. }

RICHARD WALKER.

This foreign gentleman happened to visit the Radcliffe Infirmary soon after my conversation with Mr. Walker. He has deduced some peculiar opinions on the nature of phthisis from his researches on the blood taken from persons labouring under the disease: these opinions, I understand, are quite different from that which I have proposed, and indeed utterly foreign to it.

It is well known that the symptoms of phthisis have been greatly aggravated in some patients who have been made to respire

spire oxygene air. Mr. Fourcroy describes the result of the trial of oxygene air upon twenty patients, of whom he saw eleven himself. After a few flattering appearances, which inspired them with very sanguine hopes, they were all sensibly the worse for this treatment, and as sensibly relieved by abandoning it. ‘Even amid their self-congratulations,’ says he, ‘several signs admonished the attentive physician that their hopes were ill founded. The skin was dry and hot; the face took fire and became of a more florid red, *s’allumoit et se coloroit d’un rouge plus vif qu’il n’étoit auparavant.*’ This heightening of the colour by the inspiration of oxygene air depose strongly in favour of the opinion I am maintaining. Since the complexion, already more florid than natural, is heightened by the addition of oxygene, may we not conclude that the first gradation is also owing to an excess of oxygene. ‘The symptoms’ Mr. Fourcroy goes on to inform us, in a fortnight or three weeks after the first seemingly favourable effect of the oxygene air

air ‘ became all at once more severe; the
‘ change was indicated by a dry convulsive
‘ cough, spitting of blood, a sensation of
‘ burning heat and sharp pain in the thorax, a
‘ fever almost acute and threatening to be-
‘ come inflammatory, by agitations in all
‘ the members, restlessness, and thirst.
‘ It was necessary to bleed, to give anti-
‘ phlogistic and sedative remedies, and the
‘ patients shewed great unwillingness to in-
‘ spire the oxygene air. When these violent
‘ and alarming symptoms were allayed by
‘ proper treatment, the disease resumed its
‘ ordinary form, and the fever appeared with
‘ its quotidian type; the expectoration be-
‘ come purulent again. In its 4th stage the
‘ disease made a quicker progress than usual.
‘ This accelerated progress, the symptoms of
‘ inflammation, the uneasiness, the oppression,
‘ the burning (*ardeur*) of the lungs, the stop-
‘ page of the expectoration, the acute hæ-
‘ moptysis, all these phænomena were ma-
‘ nifestly owing to the use of oxygene air.
‘ They equally took place in eight patients
‘ who were not so far gone as the others;
‘ and

‘ and it was necessary to abandon this mode
 ‘ of treatment—the patients themselves in-
 ‘ deed desired that it might be abandoned *.’

In the appendix to one of Dr. Priestley’s volumes on air, some cases are mentioned of phthisis, in which the patients were sensibly relieved by breathing common air largely mixed with carbonic acid air. Dr. Percival tells us that he has ‘ administered
 ‘ fixed air in a considerable number of cases
 ‘ of the phthisis pulmonalis, by directing
 ‘ his patients to inspire the steams of an
 ‘ effervescing mixture of chalk and vinegar, or vinegar and potash. The hectic
 ‘ fever has been considerably abated, and
 ‘ the matter expectorated has become less
 ‘ offensive and better digested. I have not,’ he adds, ‘ yet been so fortunate as to effect
 ‘ a cure. One phthifical patient has, by
 ‘ a similar course under Dr. Withering’s
 ‘ care, entirely recovered, another was rendered much better, and a third, whose

* Annales de Chimie, iv.85.

‘ case seemed to be truly deplorable, seemed
‘ to be kept alive by it more than two
‘ months *.’

We cannot be surprised that these experiments should not have been attended with greater success, if we consider that those who made them could not at that early period be enlightened by the grateful dawn of a probable theory; that having no well-defined end in view, they could not vary their means with sufficient intelligence; and that, where the apparatus was so awkward, sufficient perseverance could not well be expected. If our object be to lower the standard of the atmosphere, carbonic acid air will not probably be chosen for this purpose. Should it be objected, that the abstraction of the oxygene was not continued long enough for the effect to be produced in this way, it may be replied, that in Mr. Fourcroy’s experiments the application of oxygene was not probably continued much longer.

* Priestley, I. 301.

Some small probability arises in favour of this theory, from the inconsiderable number of sailors who die of phthisis, unless the common books, as those of Lind, Kouppe, Blane, &c. from which alone I draw my information, have impressed me with wrong notions on this point. Seafaring people are particularly exposed to wet and cold, the exciting causes of phthisis; a large proportion of them are, I suppose, of such an age as, according to the common estimate, is most liable to be attacked by consumption, and yet they seem to be even peculiarly exempt from this disease. In the ten years' register kept by Mr. Gorfuch, at Shrewsbury, upwards of one fourth of the deaths appear under the title, consumption. Though the bills of mortality are inaccurate in their denominations, yet their authority is sufficient to prove, that vast havoc is made by consumptions in London. From the accounts and lists of the sick and dead on ship-board, considered with the necessary attention to the great difference of circumstances, I should not conceive so formidable an idea
of

of the ravages committed among seafaring persons by this disorder; before any safe judgment can be formed, however, it would be necessary to have more precise data than I have met with, and especially to ascertain how far it is common for persons seized with consumptions at sea to die on shore.

Scorbutic persons ought not, according to this theory, to be liable to phthisis, nor phthifical to scurvy; at least it should seem, that as one of these diseases comes on, the other should retire. It does not appear to me, that we have well-ascertained facts enough here to afford a test of the truth or falsehood of the forgoing reasoning. Dr. Lind, indeed, says, that ‘persons very much emaciated with the *flux* or *consumption* are seldom or never seized with the scurvy *.’ I suppose so careful an observer would use terms accurately, and more confidence may, perhaps, be placed in his experience in the scurvy, than in that of all

* p. 508.

other writers put together. But in Mr. Ives's Journal it is said, that 'five or six scorbutic men, who had coughs, are now in deep consumptions*.' Again he says, 'ulcerated lungs is a common consequence of the scurvy†.' Dyspnoea, tightness and pain of the breast with coughing, are among the ordinary symptoms of the scurvy; nor will this appear extraordinary to a person who considers that neither the lungs, nor the left auricle and ventricle of the heart, perform their functions properly. It is therefore very possible to confound the pulmonic symptoms of phthisis with those of scurvy, though they seem perfectly distinguishable, by the absence of hectic fever in the latter case, and by their yielding to vegetables. I have already endeavoured to shew, that very frequently the immediate cause of death in the scurvy is the diseased state of the contents of the thorax; and if all circumstances be impartially considered, we shall conclude that Mr.

* p. 92.

† p. 107. May there not be *scorbutic* ulcers of the lungs?

Ives's account is not explicit enough to satisfy us, what effect phthisis produces upon the scurvy; whether the two disorders go on together, and whether the ulcerations of the lungs are such as take place in phthisis, accompanied with tubercles, &c. It would be curious to know how the slow and feeble pulse, natural to scurvy*, is modified by those causes which in phthisis render the pulse so frequent and so hard. Lind relates, from his own experience, that in the *Salisbury* man of war, in May 1747, 'when there prevailed several inflammatory disorders, particularly peripneumonic fevers, or inflammations of the lungs, all who were recovering from them became highly scorbutic †.' This observation is so far from forming a difficulty in the way of any part of the preceding theory, that it remarkably confirms what has been said of scurvy. Those who die of pneumonia die, I believe, of suffocation; the countenance appears bloated, discoloured, and li-

* Lind, p. 108.

† p. 74.

vid: even when the expectoration, upon which so much depends, goes on favourably, we may reasonably suppose that less oxygene will be received through the lungs than in perfect, health during that debility of the thoracic organs which succeeds the state of inflammation. Hence the scurvy will easily come on, when its exciting causes are at hand.

I do not know whether the emaciation preceding and accompanying phthisis will be allowed to afford me any additional presumption. Consumptions have been said to originate from the abuse of vinegar and sour fruits; but here I believe there is an inaccuracy in terms. In some instances of the disorder arising from this cause, I have observed it to affect the abdomen, and not the thorax. The stomach and bowels are probably burned by the long continued application of even weak acids; and hence atrophy ensues, as the chylopoietic viscera become unfit for the office of nutrition.

The beneficial effect of the mineral acids in allaying the hectic symptoms for a short time would be a contrary probability, if acids have not some immediate action, independent of their composition. The use of nitre in incipient phthisis, if nitre, which I doubt, be really beneficial, would also form another objection.—It would therefore be rash to place much confidence on so incomplete a theory, however strongly it may seem to be favoured by some of the principal phænomena. If I might even take it for granted that excess of oxgene is a well-ascertained circumstance in phthisis, it would still remain to be determined, before the investigation could lead to any thing useful, what rank it holds among the other deviations from a state of health observable in this disease.

Here two suppositions occur: 1. The phthical inflammation may so alter the structure of the lungs, as to cause them to transmit a more than ordinary portion of oxgene to the blood; or, 2. Some unknown

known cause having enabled them to transmit, or the blood itself to attract, more oxygene, an inflammation of the lungs might ensue *.

The following observations of Mr. Lavoisier may perhaps assist the reader's reflections, as well as illustrate several points of the preceding disquisition. That great philosopher had confined a Guinea pig for an hour and quarter in 248 cubic inches of

* During sthenic inflammation, does not a too rapid combination of oxygene take place, of a kind similar to the secondary combination described above, in consequence of which blood changes from florid to black? May not the heat attending inflammation depend upon this secondary combination? Does not the livid colour succeeding violent inflammations countenance this supposition? or is that colour solely owing to stagnant extravasated blood, in which the oxygene undergoes this secondary combination? We must seek the explanation of the mechanism of such changes in the principles of chemistry; and the knowledge of this mechanism will not fail to be useful. If, for instance, the above hypothesis were true, it would not be difficult to draw from it some practical inferences respecting the prevention of gangrene.

oxygene air. ‘ Towards the close of the
‘ experiment,’ says he, ‘ the animal ap-
‘ peared to suffer considerably. Neverthe-
‘ less, only a very small portion of the air
‘ was vitiated, i. e. converted into fixed air ;
‘ there remained, after the animal was re-
‘ moved, much more vital air than is ne-
‘ cessary to constitute a salubrious atmo-
‘ sphere ; this circumstance,’ viz. the distress
and death of animals confined in oxygene
air, long before it had become unfit for re-
spiration, ‘ had been noticed by Dr.
‘ Priestley. Having occasion to repeat some
‘ of his experiments, I chose Guinea pigs
‘ for the purpose: the air in which they
‘ were confined, was nearly pure ; it did not
‘ contain above five or six parts in an hun-
‘ dred of azotic air. Although the animals
‘ lived much longer in this than they would
‘ have done in an equal volume of atmo-
‘ spheric air, they nevertheless all died
‘ long before it was completely vitiated ;
‘ and other animals introduced into the air
‘ in which they had died did not appear to
‘ suffer, at least for some time. It is not

‘ therefore for want of air fit for respiration that animals die in vital air, but from some noxious effect of that air.

‘ Dr. Bucquet assisted at some of my experiments ; and we opened the animals that had died in vital air. In every instance death seemed to have been occasioned by an ardent fever, and an inflammatory disease. The flesh was of a *very red* colour ; the heart livid, and turgid with blood, especially the right auricle and ventricle ; the lungs were very flaccid, but *very red*, even externally ; they were also turgid with blood.’ (*Mem. de la Société R. de Medicine*, t. V. p. 575—576.)

The unusual animal heat, which must have been generated in these experiments, the stimulant power, which, independently of the heat, oxygene confers upon the blood, that irritability which it communicates to the solids ; all these causes might easily produce the inflammation observed by Mr. Lavoisier. Now may not the flower and differently

differently modified inflammation of the lungs, in phthisis, originate from a smaller excess of oxygene thrown into the system in a more gradual manner?

According to the former of the two suppositions stated above, we might hope sometimes to succeed in curing the disease by withholding oxygene, and giving the pulmonary ulcers an opportunity to heal; according to the second, the disease would be still more in our power; by removing the cause that produces and continues it, we might, with greater certainty, expect the inflammation to subside.

Of these hypotheses, I think it some recommendation that they lead to a project totally different from the nugatory modes of practice heretofore employed. The treatment they suggest is so obvious, that it is scarce necessary to add a syllable on the subject. Fruits, herbs, milk, &c. with all their cooling and all their occult qualities besides, have never, I suppose, effected a cure of phthisis; nor am I acquainted

with any reason capable of satisfying a person at all solicitous in forming his opinions to discriminate truth from falsehood, that they have ever contributed towards a cure. While the disease is forming, indeed, at which time the disorder seems to be highly inflammatory, an opposite diet may accelerate its progress. But there will, probably, be little difficulty in prevailing upon men of reflection to avoid both a vegetable and a stimulating diet; and to put their phthical patients upon such a diet as, according to the idea of that disease already so frequently repeated, shall tend to produce the scurvy. Not only salted meat, but an oily diet, may be tried. It will not however, I imagine, avail us much solely to cut off the supply of oxygene by the stomach. The lungs themselves being diseased, and also being the most copious source of oxygene, it would be most advantageous to supply them with an air suited to our purpose; such an air should be mixed either with an additional quantity of azotic or with hydrogenic air, which seems to have no irritating quality, and has been found to have the

power

power of darkening the colour of the blood. We cannot expect benefit from the air of a crowded room, since its temperature may counteract the effect of its diminished proportion of oxygene. It is possible, but by no means certain, that the steams abounding in such a room, which have been complimented with the title of *putrid*, may be injurious to consumptive persons. Till some means of lowering the standard of atmospheric air, without adding to it any thing hurtful, shall be contrived, we may remove phthifical patients out of those airy spacious apartments which of late have been thought salutary in all diseases indiscriminately. They may at least sleep in confined rooms; and the more confined the better, provided a cool temperature be maintained.

Here it may be asked, with reference to this practice, whether consumptions destroy a larger proportion of the inhabitants of the town or the country. I shall have occasion to say a few words on this subject below.

below. In the mean time, we can scarce expect any effect from the state of the atmosphere, since it appears to contain an equal quantity of oxygene air in the most populous city and the closest weather *.

It has sometimes been supposed that hæmoptoe and phthisis have been produced by quicksilver †; and this fluid metal has been

* According to the experiments of Mr. Scheele and Cavendish.

† In the scurvy, preparations of quicksilver in extreme small quantity produce a copious and dangerous salivation, almost always attended with bloody stools. (*Lind, p. 111.*) A tendency to salivation is frequently observed in scorbutic persons, independent of quicksilver. The chemical condition of the system, I suppose, is the cause of the first mentioned appearance. But in what this consists, I cannot form any satisfactory conjecture at present. I think it also not presumptuous to expect from chemistry the explanation of the peculiarity, which appears in some persons, whose skin is no sooner touched with quicksilver ointment than it is felt in the salivary glands. Most idiosyncrasies depend probably upon something peculiar in the chemical composition of the system. Of such effects there must be determinate causes; and I see none so likely

been imagined to increase the momentum of the blood so much as to break through the loose and tender vessels of the lungs. This is a very clumsy account of the beginning of phthisis, and such gross mechanical ideas are doubtless inapplicable to the motions of the living system. Quicksilver is taken in oxygenated, and thrown out reduced; and the properties of oxygen, and its ascertained connection with the functions of animals, seem to afford a far more appropriate explanation of the phenomenon.

The fact, if it might be explained in this way, would corroborate a supposition which is suggested by a common appearance; the supposition is, that the hyper-oxygenated state of the system precedes those symptoms which characterize phthisis. I am inclined to believe that the fatal and peculiar inflammation of the lungs, whether indicated by acute pains in the chest, spitting of pus, &c.

likely as this: I do not, therefore, wonder that these phenomena have hitherto appeared so unaccountable; for we have totally wanted data to explain them.

is posterior to the accumulation of oxygene. In persons only threatened with consumption, and before any formidable signs of disease in the thorax occur, we often observe hæmorrhages from the nose (in which I have thought the blood preternaturally florid) as well as a remarkable brightness of the complexion. Thus Mr. Portal, the celebrated anatomist, and one of the latest observers, speaking of persons, *menacés de tomber dans la phthisie*, mentions the *rougeur souvent habituelle de leur visage**: this indeed is a very common place observation.

I add a general consideration, which may possibly induce the physiologist to weigh the pretensions of the foregoing hypothesis with deliberation. The functions of the lungs seem to have been determined by modern experiments with great precision; they are destined to transmit oxygene to the blood, while at the same time a quantity of carbone and hydrogene passes off in a contrary direction, and uniting with the oxy-

* *Esprit des Journeaux*, Mars, 1792, p. 362—2.

gene of the atmosphere, forms carbonic acid and water. Hence one might conjecture that the lungs will be apt to deviate from an healthy state in two opposite ways; in one of their morbid conditions, the combinations that take place within the thorax will be impeded; in another, they *may* be carried on to too great an extent; and then, the lungs being the principal focus of animal heat, they might be injured by being constantly exposed to too high a temperature, or by having too much oxygene offered to the attractive power of their own substance.

Here I foresee, without dreading, a specious objection. ‘ Since the lungs are diminished during the progress of phthisis, is it likely that they should, with a narrower area, carry on, to too great an extent, the combinations to which they are destined? Is it not also to be expected that the inflammation should render them less permeable?’ It might be replied, that the great mischief seems to be done before they are either

much thickened by inflammation, or wasted by absorption. Besides, it is common enough for opposite conditions to exist either throughout the whole system, or in a particular organ, at different periods of a disease, as excess of action is followed by debility. I however think it probable that instances have occurred, where the loss of a considerable part of the substance of the lungs has checked the progress of the disease. I knew a person who died, after a tedious struggle, of phthisis. The ribs on one side were pressed quite inwards, in consequence, I suppose, of the destruction of the corresponding lobe of the lungs. From this time, the disease, which had at first proceeded at its ordinary rate, went on wonderfully slow; and it was not till several years after the depression of the ribs that the patient died, without ever having had a distinct and long continued intermission. However this may be, in the generality of cases actual observation shews the objection to be groundless. In the various shades of livid colour, which are produced by a total

or partial exclusion of that portion of oxygene, which enters the blood by way of the lungs, we have nearly as good a criterion of any considerable deficiency of this principle as we can desire. Now I believe it seldom or never happens that a livid suffusion overspreads the countenance of a phthifical patient: *les rougeurs*, says Mr. Fortal, *augmentent presque jusqu' au dernier moment*; and in proportion as the dark-red asthmatic flush is a stranger to this disease, we may infer that even the phthifical dyspnœa does not prevent a full supply of oxygene.

The more you reflect, the more you will be convinced that nothing would so much contribute to rescue the art of medicine from its present helpless condition, as the discovery of the means of regulating the constitution of the atmosphere. It would be no less desirable to have a convenient method of reducing the oxygene to 18 or 20 in 100, than of increasing it in any proportion. The influence of the air we breathe is as wide as the diffusion of the
U 2 blood.

blood. The minutest portions of the organs of motion, sense, and thought must be affected by any considerable change in this fluid. Whether it be that the brain must be washed by streams of arterial blood, or that the action of every organ is a stimulus to the system in general, and consequently to every other organ in particular, it is certain, that when the access of oxygene is cut off from the lungs, the functions of the brain cease: perhaps there may be a mixture of azotic and oxygene airs more favourable to the intellectual faculties than that which is found in the atmosphere; and hence chemistry be enabled to exalt the powers of future poets and philosophers. That diseases of excitement on the one hand, and debility on the other might be cured almost solely by a proper air one can hardly doubt, as well as several disorders at present highly dangerous or desperate, which one cannot, upon the faith of any obvious phænomena, refer to either head. The materia medica might, therefore, undergo a still greater reduction, than it has lately undergone in consequence
of

of the purification of medicine from its grosser absurdities ; and hence the treatment of diseases be at once rendered infinitely more pleasant and more efficacious.

The extreme frequency of consumptions in Great Britain seems to have been justly ascribed to the variableness of the climate. A few reflections will, perhaps, render it probable that the cause is adequate to the effect, at least that it is peculiarly calculated to excite pulmonary inflammations, with the concurrence of other circumstances. Climates of equable warmth appear to be most favourable to health, and much the most congenial to the human race. Next to these may be placed those climates which have fixed seasons ; the most destructive are the variable. In England we seldom enjoy any continuance of settled fine weather, except towards the close of summer and the beginning of autumn, and even then we are frequently baulked in our expectations of fine weather. The sudden changes that take place during three fourths of our year

may be regarded as no less prejudicial to the health, than disagreeable to the feelings; and our terrors of *catching cold*, which have frequently appeared ridiculous to foreigners, are really better founded than we ourselves are apt, most of us, to apprehend; colds, in their consequences, proving fatal every year to multitudes both of the young and the old; to the former principally by giving rise to consumptions, to the latter by producing pulmonary diseases of a different character.

We cannot hope entirely to escape the unpleasant sensations, or to ward off the fatal effects, occasioned by this caprice of our climate. But by understanding how colds are caught, we may be induced to take certain simple, but useful precautions. One may with the greater propriety embrace an opportunity of disseminating this sort of information, as the manner in which colds or inflammatory cattarrhs is taken, though now in my opinion perfectly ascertained, is far from being generally understood by the
members

members of the medical profession; and if any person, not belonging to that profession, should suspect this to be a wanton paradoxical assertion, he may find in the case of opium, and of the cool treatment of small pox, &c. instances equally striking, where one generation of pathologists passed away after another, without being able, in the case of opium*, to perceive the plainest appearances, or, in that of small-pox, to draw the simplest conclusion. So servilely imitative an animal is man! so loath to employ his own powers of perception and thought!

* It is curious to see what pains medical writers have taken to imagine hypotheses, either out of mere complaisance to the term, *narcotic*, or because opium is a drug in Christendom, and wine an article of diet, rather than suffer themselves to see that opium makes a man merry or drunk, then lays him asleep, and afterwards causes him to awake with an head-ache, in the same manner as wine. One cannot compare Haller's clear and satisfactory parallel of wine and opium, published in 1769 (*El. Physiolog. t. V. p. 610—11*) with Cullen's perplexed and hypothetical doctrine of opium, and his whole article *sedantia*, published in 1789, (*Mat. Medica, t. II. 217, & seq.*) without a sense of humiliation.

When

When any part of the body has been exposed to cold, it is liable to be much more affected by heat and other stimuli than before the exposure. Of this the method of treating frozen limbs in cold countries affords a beautiful and decisive proof. Were a frozen limb to be brought before a fire, or immersed in warm water, a violent inflammation would come on, and speedily terminate in mortification. They therefore take snow to rub the parts benumbed with cold, and very gradually expose them to a warm temperature. The glow, after coming out of the warm bath, is entirely owing to the lower temperature to which the body has immediately before been exposed, or, what amounts to the same thing, the power of water to conduct heat away from the body faster than air. The pungent pain felt upon holding an hand much chilled to the fire is another exemplification of the same principle, which seems to be one of the most general laws of animal nature. In like manner the cessation of action and thought during

sleep accumulates a power of thinking and acting with more energy than before we fell asleep. Even Dr. Cullen, whom the prejudices of old age and the pride of celebrity conspired to hinder from receiving this doctrine in its full extent, in his last work expresses himself upon the article of sleep with a precision that is not always found in the theoretical part of his writings; ‘a state of sleep,’ says he, ‘subsisting for some time, induces a state of the system *more ready to be affected by stimuli of all kinds* *.’ The latter part of the sentence is a very accurate and luminous interpretation of the common expression, *that we are refreshed by sleep*.

* *Materia Medica*, II. 228. In his little book on physiology some ingenious hints towards this doctrine will be found: Brown doubtless profited by these hints, and, I fear, without due acknowledgment; the old Professor, on the contrary, who had gone into other theories, which he outlived, could not bear to think that a man, who had been almost his servant, should have matured his ideas into a system highly ingenious and partly just.

Now after the application of cold, which, according to circumstances, produces a greater or smaller diminution of the actions of the living system, and at length sleep itself, there may be an infinite number of gradations between a fatal inflammation and a transitory glow, and this according as the previous cold and the subsequent heat have varied in intensity; but whatever be the degree, the effect depends on the same principle. By respiring a cold atmosphere the same thing happens to the nostrils, fauces, lungs, as to the external surface of the body upon going into a cold bath; and if we pass suddenly from such an atmosphere into a warm room, what happens to the skin will in some degree happen to the membrane lining these cavities; a glow or inflammation will ensue, according to the difference between the two temperatures and the length of time passed in the cold. When the application of cold or moisture to a superficial part only is succeeded by an inflammation of the respiratory cavities, the consent of the whole system

system easily explains this remote local affection. The cause of disease pervades at once and feels as it were, or searches the whole body, but affects only in a degree to draw our notice to the organ which from habit or structure is most tender. Should any other part, from previous circumstances, have been rendered more sensible to its influence, we shall in consequence have a sore throat, a diarrhœa, or the rheumatism, in place of a catarrh. Children are so susceptible of inflammations that a great part of the mortality among them is, as far as I have observed and can judge, to be ascribed to the ignorance of mothers and nurses of the power which even a moderate change of temperature, if suddenly made, has to affect their tender and irritable frame. Whenever accurate registers of the mortality of the human species, in climates equably warm, shall be kept, I expect that not half so many infants will be found to die as in Britain. Hence, in part, the populousness of such countries: those gardens of the

earth being equally calculated to rear and support inhabitants.

In catarrh therefore, whatever be the degree of inflammation, from the slightest affection of the nostrils or chest to the most acute pleurisy, it never takes place, unless we pass too suddenly into a comparatively warm atmosphere, or apply a stimulus equivalent to heat. The feverishness, the internal glow, the dryness of the nostrils, the *huskiness* of the bronchiæ, are never felt while we remain in the cold; they speedily come on after entering a warm room. Persons who can recollect their past, or will attend to their future sensations, will easily be convinced of the truth of this simple and salutary theory. Formerly, when I first heard it, I must own that I durst not believe it on account of its simplicity*. It

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* As far as I can judge from recollection, the following latent hypothesis was a great cause of doubt. Anatomy seemed to demonstrate a wonderfully complicated structure in the human body. It was obvious to ima-
gine

was not till having been made more attentive to facts by a desire to determine the merits of the different theories of catarrh, that I was irresistibly convinced by my own personal experience of the justness of that shortly stated above, and which indeed requires but very few words for its exposition. Frequently after riding for hours in the rain, especially during summer, I have felt a glow infinitely more vivid than in coming out of the cold bath. This glow was owing partly to the temperature of the atmosphere, and partly to the slight exercise of changing cloaths. I have sometimes made other persons attentive to the progress of the phenomena, and nothing has appeared more evident than that during exposure to wet or

gine that the functions or use of this complicated structure must be complicated in proportion. This is a false conception: the living body is in reality infinitely more simple than it appears at first view, at least as to the actions and diseases of its parts; and the structure is but a constant repetition of nerves, blood-vessels, cellular substances, fibres, and fat. The anatomical knowledge of a surgeon, indeed, must be minute, but it is not abstruse; not much more so than that of geography. It is perfectly analogous to subterraneous geography.

cold,

cold, no tendency to inflammation is perceptible, but that subsequent heat, exercise in the dry, and stimulants, produce the glow or inflammation. By keeping quiet and cool for some time after being wet in summer, and by avoiding a sudden transition into a warm temperature in cold weather, and by temperance in both cases, those inflammatory diseases, for which cold only prepares the system, may be easily avoided; and any person, by acting upon these principles, may have at pleasure a slight or a violent catarrh, or no catarrh at all.

The popular treatment, therefore, of colds during their early stage is just as prejudicial, as the ancient hot regimen during the small-pox. Warm or spirituous liquors, warm close rooms, and a weight of bed-clothes cannot but aggravate the symptoms. The same may be said of Mudge's inhaler, which though certainly serviceable in the asthenic catarrh, or *catarrhus senilis*, I have oftener than once observed sensibly to
heighten

heighten the inflammation at the commencement of a common cold*.

It

* *Catarrhum igitur e frigore esse, calore solvendum, gravissimus error est. Contrà, frigus nunquam nocet, nisi ubi ejus actionem calor excipit.*—*Catarrhus æstate toties incidens, ubi sexcenties causa ejus a frigore supra repeti nequit, a calore potest; contagiosus nunquam, communis sæpe; frigoris egens; non omnino frigori, calori protinus succedens;—observationem condem firmant.*

Such is the doctrine of Brown, (Elem. Medicin. II. 42, 43) a noted author and teacher, as he is styled by Cullen, (Mat. M. II. 235.) who was very desirous to persuade himself that the adversary whom he notes, and against whom he protests, was contemptible. For my part, I consider his doctrine of the effect of stimulating powers applied to accumulated excitability as the only specimen of extensive reasoning in pathology, calculated to afford any satisfaction to a just thinker. He avoids those unmeaning or vague terms, that had been before so much employed to shelter ignorance from their employers and from others; he appeals to phænomena of the living system obvious to the senses; and adopts such principles of reasoning that, if he has not always discovered the truth, he is seldom forsaken by the spirit of a philosopher.

I beg leave to add—not for the sake of propitiating medical orthodoxy, but of preventing misapprehension—that in his system of opinions and practice, as delivered either in his lectures or writings, I have always found full as
much

It has been, I believe, unfortunate for the inhabitants of this country, that we are not subject

much to reject as to receive. His directions for the application of stimuli often seemed contrary to his principles; and, I believe, he led his followers into dangerous mistakes on this important point. I have nothing to say in defence of his personal conduct, which more than any thing else caused his doctrines to be condemned unheard, as if the grossness of a man's manners affected the conclusiveness of his arguments. If his imprudence, however, was highly blameable, and his arrogance intolerable, the liberal will allow something to the deep consciousness of neglected merit, and to the irritating sensations attendant upon declining health: and posterity may reproach an age, in which a man, possessed of powers so superior and so nobly exerted, was brow-beaten, defamed, almost persecuted, and left to perish in extreme penury.

It is to be lamented that Dr. Brown's writings should be so little known to those who are secretly influenced by his opinions, which in one way or other have been so widely diffused as to affect almost the whole practice of medicine in Great Britain. His mistakes a dispassionate man would easily distinguish and avoid; but as he certainly did not err wilfully, I know not why they should be held up as criminal, or censured with greater asperity than the exploded theories of Boerhaave or Cullen.

From conversation with the author of the *Botanic Garden*,

subject to such a continued severity of cold, as should oblige us regularly to fortify ourselves by warm cloathing. By linen, worn exclusively, we lose more in health than we gain in comfort; which comfort is, perhaps, after all, merely imaginary; for there is hardly an instance in which the skin does not soon reconcile itself to woollen, though there is no necessity for placing it next the skin, and cleanliness is just as much in the power of the wearers of woollen. The most simple method, as well as the

Garden, who is no less eminent as a physician than a poet, I find that he entertains similar sentiments concerning the Brunonian doctrine. Several of Dr. D.'s friends have indeed assured me, that he had discovered these principles many years ago, and reduced them to a regular system. Indications of such a system appear in some of the notes to his beautiful poem, but the complete treatise has never got beyond the circle of his friends; a rare example of modesty or indifference to fame! It is much to be wished that the anticipation may not finally deprive us of the treatise: some peculiar illustrations it must have, every one of which would enlighten the philosopher and guide the physician. The authority of the writer too would secure the doctrines a fair hearing, and his ample experience would inspire confidence in his reasonings.

most effectual, to avoid the influence of sudden changes of atmospherical temperature is to wrap the body in substances that conduct heat slowly. Both for this reason, and because it is so much less unpleasant, when moist, than linen, flannel should be worn at least above linen during every season in Great Britain; and those who feel it necessary may double it during the winter, spring, and beginning of summer.

In children it is of the utmost consequence to keep the body cool, but never to suffer it to be cold. Thus, without being enervated, they may escape the fatal consequences of heat succeeding quickly to cold; for it is not true, as seems, in consequence of an analogy more or less distinctly conceived, to have been frequently imagined, that cold hardens children as it hardens steel.

Persons advanced in years and subject to the asthenic catarrh, suffer from the immediate application of cold or moisture, which

which is but cold in another form. The transition to a cold or damp air will immediately affect them: I have observed the symptoms to commence as soon as one breath of such air was inhaled; and in some cases they are very quickly freed from dyspnoea, wheezing, defluxion, &c. upon removing into a warm dry air; insomuch that there is a state of the disease when a person may take and lose this sort of cold several times a day. I am afraid dress alone will never prove so effectual a preservative against this as against the other kind of catarrh. If some portable apparatus for warming the air before its admission into the lungs could be contrived, this perhaps would ward off those pulmonic attacks, which though only dangerous at a certain age, or in a state of considerable violence, are at all times distressing enough to be numbered among the innumerable evils of a moist and variable climate.

A very recent writer, by no means deficient in acuteness, questions, or rather shews

an inclination to question, the baneful effects arising from the variableness of our climate.

‘ There is, perhaps,’ he observes, ‘ no country where coughs are so frequent as in Great Britain, which is generally imputed to the excessive variableness of the climate. But this cause seems to have no ill effect upon the health of wild animals, who are exposed to the same uncertain weather.’—This variableness ‘ does not prevent many vegetables and animals from arriving at their utmost perfection.

‘ The blooming complexion of our peasantry—the permanence of their good looks, and their strength and activity strongly shew that the climate is not unwholesome to those properly educated. Strangers, however, often suffer by the sudden transitions from heat to cold, and from dry to moist, which are so frequent. And those natives suffer likewise who are delicately brought up, and who, from being generally confined in warm apartments, seldom feel the natural temperature of the
‘ air,

‘ air, and acquire in this northern region
 ‘ constitutions adapted to Italy.—They ac-
 ‘ quire’ (from change of weather) ‘ catarrhs,
 ‘ pleurifies, consumptions, sore throats, fe-
 ‘ vers, and other diseases.

‘ But these ailments ought rather to be
 ‘ imputed to a delicate constitution, acquired
 ‘ by an improper mode of life, than to the
 ‘ climate.’—He goes on to say, that an in-
 fant born healthy, and hardily brought up,
 would be nearly as little liable to injury by
 the variableness of the weather as a young
 fox *.

Several reflections connected with the pre-
 ceding observations are suggested by these
 passages. 1. If the wild and unhoused ani-
 mals are less subject to pulmonic diseases,
 it is because those diseases are rarely produced
 by natural heat ; or by any stimuli, to which
 such animals are exposed. There cannot
 well be a more decisive proof, than this ge-
 neral fact, of the truth of the theory. Upon

* Moore's Essay on the Materia Medica, 1792, p.
 280, &c.

this principle we may account for an appearance that has often struck the pathologist. The catalogue of the diseases of wild animals is extremely short. Domestic animals being less subject to the action of artificial heat and stimuli than man, but more so than wild animals, the effect is found in this instance also to be in proportion to the cause. Thus, as the author just quoted well observes, except a few domestic animals, such as are stabled in warm buildings, or tended with human care, others are little liable to catarrh*. The most tender seem liable to few, except disorders arising from the excess, or defect, or unwholesome qualities of their food, and these are chiefly disorders of the alimentary canal; by far the greater number would survive to a good old age, if left to themselves, and not, like half the human species in our climate, sink back into the tomb, as soon almost as they have caught a few glimpses of the light of heaven. 2. If delicate persons are subject

* *Coughing*, the most equivocal of symptoms, may arise from a diseased liver, &c. &c.

to diseases and dangers in England to which they would not be subject in Italy, it is evident that the difference between the climates of England and Italy is the cause of these diseases and dangers. 3. I am much afraid that the greatest mortality of children is among those hardily brought up. It is, I conceive, impossible for a man of humanity, or even of common reflection, to read the following passage without shuddering at the extreme wretchedness, and the wide extent of that wretchedness, which it announces.

‘ Poverty, though it does not prevent the generation, is extremely unfavourable to the rearing of children. The tender plant is produced, but in so cold a soil and so severe a climate, soon withers and dies. It is not uncommon, I have been frequently told, in the Highlands of Scotland, FOR A MOTHER WHO HAS BORNE TWENTY CHILDREN NOT TO HAVE TWO ALIVE*.’

In England the children of the poor are swept away by hundreds and by thousands ;

* Smith’s Wealth of Nations, I. 12c.

and the axiom of medicine, *penuria morborum causa*, is, I believe, even more strikingly verified in the case of poor children than of their parents. But I suppose from what follows, that the author means by *hardily brought up*, such as are not enervated. 4. I am much afraid, however, that the peasantry, after weathering the early inclemencies of their station, enjoy no such advantage of freedom from pulmonic complaints as he seems to imagine. To mention one or two probabilities to the contrary: an inconsiderable city, and a thinly inhabited country, yield to the Oxford infirmary but a scanty supply of patients; yet I have been told, that for a great number of years past, there have been almost always among the number some ill of phthisis, or threatened by that disease. Here it may be remarked, that neither confinement in close rooms, unwholesome fumes, or an unfavourable posture, contribute any more than an effeminate education, to the production of consumptions. Again, among the peasantry of Warwickshire and Staffordshire, I under-

stand that consumptions are extremely frequent ; not less so than among the Birmingham manufacturers. And who has not often met with phthisis in the most airy situations ?

Among the richer class this fatal disease would, perhaps, be less common, did they not by a strange infatuation take the most effectual steps to contract it. If a greater proportion of females fall victims to it, is it not because, losing sight more than men of its primary purpose, they regulate their dress solely by fantastic ideas of elegance ? If happily our regret should recal the age of chivalry, to break the spell of Fashion would be an atchievement worthy the most gallant of our future knights. Common sense has always failed in the adventure ; and our ladies, alas ! are still compelled, whenever the Enchantress waves her wand, to expose themselves, half undressed, to the fogs and frosts of our island.

TWO MEMOIRS

TRANSLATED FROM THE FRENCH

OF

DR. GIRTANNER*.

MEMOIR I.

THE irritable or contractile, improperly called the muscular fibre, when separated from the animal or the plant, preserves its irritability for some time, and continues to contract upon the application of any stimulus. It retains also this property when it is cut into pieces, as is observable on dividing the proboscis of a butterfly, or the stamina of plants. Each piece continues to contract, which proves that the smallest portion of the fibre possesses its

* N. B. Part of the first memoir is omitted.

own peculiar irritability, independent of the other portions. The fluids in animals and vegetables are endued also with this property as well as the solids. Their irritability consists in coagulability, which coagulability in fluids is subject to the same laws as the irritability of the fibre. This is a new discovery; and it forms the basis of many important truths. The degree of irritability of both solids and fluids is constantly changing; it varies according to the age and habit of the same animal and the same plant, and according to the sex, organization, and size of the different individuals. It is encreased by the absence of the common and habitual stimuli, and is weakened by the too frequent application of the same stimulus, or by the application of a stimulus too powerful. We may distinguish three different states of the irritable fibre, or three different degrees of irritability of which it is susceptible.

1. The *state of health*, which is peculiar to each individual, and which I shall call

call the *tone* of the fibre, after a term of Stahl.

2. The *state of accumulation*, produced by the absence of habitual stimuli.

3. The *state of exhaustion* produced by the too powerful operation of stimuli.

The *state of health*, or the *tone* of the fibre, consists in a certain quantity of the irritable principle necessary to its preservation. To maintain this state, the action of the stimulus must be strong enough to carry off from the fibre the surplus of this irritable principle, which the lungs and the circulation of the fluids are continually supplying. For this a certain equilibrium is necessary between the stimuli applied and the irritability of the fibre; in fine, that the sum of all the stimuli acting upon it may be always nearly equal, powerful enough to carry off from the fibre the excess of its irritability, and not so strong as to carry off more than this excess. It is in this equilibrium

librium between the acting stimuli and the irritability furnished by the lungs and the circulation, that the *health* or *the tone* of the fibre consists. When the sum of the stimuli acting upon the fibre is not great enough to carry off all its excess of irritability, the irritable principle accumulates in the fibre, and then it is found in that state which I call the *state of accumulation*; the irritable principle accumulates in the fibre, its irritability is augmented, and the stimuli produce much stronger contractions than when the fibre only retains its tone. Hence it is that in opposing an obstacle to the movements of the hedyfarum girans for any length of time, the movement becomes considerably stronger after the obstacle is removed.

When the sum of the stimuli acting upon the fibre is too great, the fibre is deprived not only of the excess of its irritability but also of some portion of the irritable principle necessary for the tone of the fibre, or, more properly speaking, the fibre loses more irritability

irritability than it receives, and of course in a short time finds itself in a *state of exhaustion*, and this exhaustion will be either *temporary* or *irreparable*.

In the state of *temporary exhaustion* the fibre loses its tone, and fails for want of irritability. The application of a stimulus, while it is in this state, will not make it contract. Provided the stimulus be not very strong, it will produce no effect at all, but in a short time the irritable principle will accumulate afresh in the fibre, and then it will again contract. It is only by little and little that the fibre recovers its irritability. This truth, I dare venture to say, is as new as it is striking. It unfolds a vast number of phænomena hitherto inexplicable. Let us observe, for example, the motion of the heart; the heart contracts from the stimulus of the blood, and impels the blood through the arteries; it then again dilates, and the blood enters. But the heart does not contract itself immediately upon the first impression of the blood. Its irritability

lity having been lessened by the preceding contraction, it requires half or three quarters of a second before the irritability of the heart shall have accumulated to such a degree that the new stimulus can act upon it. It is impossible to explain the motion of the heart upon any other principle. Haller has indeed very well explained the motion, on the principle of the irritability of the heart; but he was never able to answer the famous objection of his opponents, who said, If the blood acts upon the heart as a stimulus, and its contraction is the consequence of such action, how comes it that the heart does not contract as soon as the blood enters it, but that it flows in some time before the contraction is renewed? Why does not the effect immediately follow the cause? Haller could never answer this objection, nor several others of the like nature, inasmuch as he was a stranger to the laws of irritability. The menstrual discharge in women is explained on the same principle. The stimulus of the ovaries acting continually in women after the age of

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puberty

puberty (as I shall prove elsewhere) nevertheless does not produce its effects till the end of eight and twenty days; because this period of time is necessary for the uterus, in its state of health, to accumulate its irritability in sufficient quantity for the stimulus to act; the discharge ceases after the irritability of this organ has been diminished and returns with the returning irritability. All the periodical motions in animals and plants, as well as their periodical diseases, may be explained upon the same principle; that is to say, any stimulus which is always present, and continually acting upon the fibre, produces no sensible effect, till the exhausted irritability of the fibre shall have been accumulated afresh. The periodical motions in organized bodies depend on the alternate exhaustion and accumulation of the irritability of the fibre. A temporary exhaustion of the irritability of the *hedyfarum girans* is produced by the heat of the sun and by electricity, according to the observations of M. Broussonet. The electrical fluid exhausts also

the irritability of the *mimosa pudica* for some time, as the abbè Bertholon has observed.

The total or irreparable exhaustion of the fibre consists in a total loss of its irritability, as in the case of gangrene. The fibre changes its colour, becomes dark or black, and subject to the laws of inorganized matter, and begins to decompose and putrefy.

A very powerful stimulus will in a very short time reduce the fibre to this state. Such, for instance, is the state of the fibre in animals killed by very strong poisons, by the bite of a rattlesnake ; in animals destroyed by a knife dipped in the juice of the aconite, or by poisoned arrows. The irritability of many insects, and of the greatest part of plants, is irreparably exhausted by the stimulus of the propagation of the species, so that they die the moment the work of generation is completed. Dr. Priestley has observed, that in exposing plants to the stimulus of air in which animal substances

have

have putrefied, it always happened that, either the plants being vigorous enough to bear the action of the stimulus, their growth was very rapid, or the stimulus proved too powerful, and the plants died; their irritability was exhausted in an instant, and their leaves became black and gangrenous.

The irritable fibre, from the first moment of its existence to that of its dissolution, being constantly surrounded by the body which acts upon it, and stimulates it, and upon which it re-acts by its contraction, it follows, that during the period of its existence the irritable fibre is in continual action; and its existence consists in action; and that it is not a passive state, as some authors have asserted. Hence, external objects having no immediate action upon the nerves, and only acting upon them, and producing their different sensations, through the medium of the irritable fibre, it is plain that the ideas we have of external objects are not conformable to those objects, but that they are varied and modified

by the irritable fibre through which they are transmitted to us. Objects, therefore, appear different according to the different states of the fibre. The irritable fibres, which are combined together in every individual, whether animal or vegetable, form a system of fibres, in which the integral parts act continually upon the whole, while the whole re-acts upon the parts, so that every stimulus which acts upon any fibre in the system will deprive that part of its irritability; but this loss will soon be repaired by the system, and every fibre will furnish, in proportion, some share of its own irritability to supply the loss in any one fibre. Thus it is that a very weak stimulus, but one that is constantly acting upon one part of the system, such as slow poisons, the abuse of spirituous liquors, &c. exhausts in the end the whole system, and produces death. For the same reason, a very powerful stimulus applied to one part of the system, such as laurel water, opium, the poison of the rattlesnake, will in an instant exhaust all the irritability of the system, destroy

destroy the animal, and leave the fibres without any irritability. I am convinced, from repeated experiments, that opium, alcohol, ammoniac, a solution of sugar of lead, sulphuric æther, destroy animals by exhausting the irritability of the whole system, and that the muscles of the animals destroyed have, by the application of these stimuli, been wholly deprived of their irritability. The effect was the same when these stimuli were applied to the muscles and stomach, and when injected into the veins of animals. I have also made very curious experiments with the same substances upon vegetables.

The irritable fibres in the same system have not all the same degree of irritability. They have different degrees of *capacity* for the irritable principle. The capacity of the fibres is in the ratio of their distance from the heart. Those equally distant have the same capacity. Every stimulus which affects one of the fibres affects the others at the same time and in like manner. Hence
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the sympathy of different and separate parts and those surprising phænomena which hitherto have been explained by the harmony of the nerves, although we see the same phænomena in the vegetable kingdom, which is deprived of nerves. These sympathetic phænomena are observable throughout organized nature. Whatever part of the polypus be touched, the whole will contract, and its arms will contract themselves by sympathy. If a worm be touched with the point of a pin, without wounding it, the whole worm will be seen to contract itself; which is a certain proof that the different parts are affected by sympathy. If the slightest impresson be made upon one of the leaves of the *averrhoa carambola*, not only that leaf but all the neighbouring ones, and frequently some of the distant ones, will contract themselves by sympathy.

When the irritable fibre has lost its tone, and fails, either from an excess of the irritable principle, or from a deficiency of this principle, it is diseased, and the system
of

of which it forms a part suffers and becomes diseased through sympathy. All the diseases of animals and vegetables may be ranged under two heads; to wit: First, the *diseases of accumulation* caused by the accumulation of the irritable principle, and by the diminished action of the habitual stimuli. Secondly, the *diseases of exhaustion*, caused by a defect of the irritable principle proceeding from the encreased action of the habitual stimuli, or from the addition of new stimuli. Under these two classes may be ranged all diseases whatever. Paradoxical as this proposition must necessarily appear to those who have not reflected on the subject, it is nevertheless true, and I shall give the most convincing proofs of it in a work I am about to publish.

Remedies remove the disease by their action upon the irritable fibre, and by exhausting its irritability, when the disease is that of accumulation; or by diminishing the action of the common stimuli, and consequently by preventing a total exhaustion,
where

where the disease is that of exhaustion. The effects of poisons are to be explained in this way.

Poisons, remedies, and in general all surrounding bodies acting only on the irritable fibre, it follows that they act upon the system in a similar manner, and that every substance capable of producing the greatest possible effect upon the fibre, that is to say, every substance capable of exhausting all the irritability both of the fibre itself and of the system in an instant, as, for instance, laurel water, or white arsenic, is also capable of producing all the inferior degrees of action, either by acting upon a fibre less irritable, or by acting upon the same fibre, but in a less quantity. Laurel water, opium, white arsenic, ammoniac, are of course both medicines and poisons capable of healing as well as of producing all maladies whatsoever without exception. And this is confirmed by a number of experiments which I have made upon different animals. This truth seems to me of the utmost importance ;

portance ; and the abbé Fontana, who made more than six hundred experiments to prove that ammoniac is no remedy against the bite of the viper, would have saved himself a great deal of time and trouble, had he been acquainted with it. If instead of applying the venom of the viper to so many animals, and afterwards applying ammoniac to the wound, he had made a single comparative experiment, and had applied ammoniac to a wound made by a lancet that was not poisoned, he would have found that ammoniac itself, applied in this manner, would have produced a disease exactly analogous to that caused by the venom of the viper, and, consequently, so far from removing the malady, must necessarily increase it, by exhausting the irritability of the fibre in a much less time than the venom of the viper by itself was capable of doing. Mr. Fontana has made more than six thousand experiments upon the poison of the viper ; he employed more than three thousand vipers, and caused to be bit more than four thousand animals, and the conclusion he

drew after this truly enormous number of observations was, that the poison of the viper kills all animals, and produces the disease by its action on the blood. But why did Mr. Fontana neglect to make the decisive experiment, the *experimentum crucis* of Bacon. It is well known that frogs, and many animals with cold blood, live a long time without the heart, and entirely deprived of blood. If therefore the poison of the viper kills animals by its action on the blood, it will not destroy frogs without blood. But experiment contradicts this reasoning. The poison of the viper will kill frogs without blood in as short a time as it kills those animals who have not lost their blood. It is not therefore by its action upon the blood that the venom of the viper destroys animals; and thus does it happen that a single experiment frequently overturns all that six thousand other experiments have apparently established. According to my experiments, poisons operate upon the blood just as they do upon the muscular fibre, by depriving it of its principle

ciple or irritability, or of its oxygene. After having made this observation upon the experiments of Mr. Fontana, I must do him the justice to add, that I have found all his experiments very accurate, and that in all those which I have repeated, the result has been exactly conformable to the account given by him ; it is in his conclusions only that he appears to be deceived.

The effect produced upon the irritable fibre by any stimulus, is in a ratio compounded of the degree of irritability of the fibre, and of the force of the stimulus. The same stimulus will produce greater contractions upon a fibre more irritable than upon one less irritable ; and the irritability of the fibre being the same, it will contract itself more upon the application of a stronger than of a weaker stimulus.

The effect produced upon an irritable fibre by any stimulus is in the inverse ratio of the repetition of its application. *Cæteris paribus*, the effect of any stimulus di-

diminishes every time its application is repeated, till at last the effect is nothing, or $= 0$. This explains the phenomena of habit, and many other phenomena hitherto inexplicable in the animal and vegetable œconomy. The *mimosa pudica*, for example, exposed to a strong wind, contracts itself; but it ceases to contract itself in obedience to this stimulus after it has been accustomed to it.

The effect produced upon the irritable fibre by any stimulus, is in a ratio compounded of the degree of irritability of the fibre and the degree of the force of the stimulus directly, and the degree of the habit of the fibre inversely. Let the force or intensity of the stimulus $= a$, the degree of irritability of the fibre $= b$, the degree of the habit of the fibre $= c$, then the effect produced upon the fibre or x will $= \frac{ab}{c}$. But all the stimuli acting in the same manner, that which diminishes the irritability of the fibre for a certain stimulus,

lus,

lus, will in the like manner diminish it for the stimulating force in general, wherefore the habit of the fibre is comprehended under its degree of irritability, or c is comprehended under b . Therefore x will $= ab$.

The effect produced upon the irritable fibre by any stimulus, or x , being always equal to ab , it follows that the value of a and b being known, the value of x is known. But admitting an unity fixed and constant, it will be easy in all cases to express by numbers the degree of irritability of the fibre and the degree of the force of the stimulus, or the value of a and b , consequently it will be easy to find the value of x . All the art of medicine then consists in the art of finding the value of x , that is to say, in finding a stimulus adequate to restore the tone of the fibre. Thus, if these principles be true, physic, which at present is an art of mere conjecture, will be reduced in time to the certainty of calculation, and after tables shall be formed to express the values of a and b , and the signs by which they

they may be known, this calculation will be so simple and easy, that it will form a part of the education of every individual. But further, the irritable fibre being the same in all organized nature, diseases and their remedies will of course be the same for all organized beings: there will then be no distinction between medicine, farriery, and agriculture, but all these sciences will be confounded, and become one, under the general name of *universal physiology*. The art of pharmacy and the science of prescription-writing will become useless; a phial of alcohol or laudanum will supply the place of that enormous quantity of drugs which crowd the shops of apothecaries. The trade of the druggist—but hold, if I continue this prophetic language, I shall only expose myself to ridicule; for, as Helvetius observes, ‘every idea very foreign to our habits of seeing and thinking appears ridiculous to us. We never value any ideas but what are analogous to our own, because we are under the necessity of esteeming ourselves only in others.’

Those

Those stimuli which I call common and habitual, because they act continually more or less upon the irritable fibre, are, *heat, light, nourishment, air, the circulation of the blood, the stimulus of generation, and the nervous stimuli.* So long as the action of these stimuli is in proportion to the degree of irritability of the system, and the sum of their action is nearly equal to the sum of the irritable principle absorbed by the lungs, and distributed by the circulation, the whole system will be in proper order, and the constituting fibres will have their tone. When one or more of these stimuli act more powerfully than ordinary, or the fibre becomes more irritable, while the degree of the action of the stimuli remains the same, the *exhaustion* of the system, and one of the diseases in its train, will be the consequence.

The absence of one or more of these stimuli will produce an *accumulation* of irritability in the system, and give birth to one of the diseases of this class. I shall speak
of

of all these stimuli separately, in order that I may be better able to explain myself.

Of heat.—The heat of the atmosphere, and of all surrounding bodies, acts upon the fibre and stimulates it. I am convinced of the stimulating action of heat from direct experiments. I have exposed small animals, such as cats, dogs, rabbits, &c. in covered vessels, to the heat of boiling water, which surrounded the vessel in which the animal was placed, so that the water could not touch it. Animals destroyed by heat in these experiments, upon dissection have been found to have lost all their irritability. Their heart and muscles contracted themselves but feebly, even upon the application of the strongest stimuli, such as electricity. It is proved by some beautiful experiments of Mr. Hope, that heat acts as a stimulus upon plants; and it is observable that plants exposed to the sun are larger, and produce more flowers and fruit than those which are less exposed to heat. Trees in general are more luxuriant which

grow in the south than those in the north. This is a proof that heat is a stimulus to the irritable fibre. The diseases of hot climates are all the diseases of exhaustion, caused by the too powerful action of the stimulus of heat. Hence the custom of taking ice in hot countries to restore the tone to the fibre, by absorbing the heat and preventing its stimulating action. This irritability of the *hedyfarum gyrans* is exhausted by the heat of the noonday sun, according to the observations of M. Broussonet; and by the experiments of M. Fontana and M. Medicus it is proved that the irritability of plants is great in the morning, diminished during the heat of the day, and little or none in the evening.

Of cold.—Cold being a less degree of heat, its effects upon the irritable fibre are in proportion to the *habit*, or the quantity which is necessary to the fibre to preserve its tone. The animals and plants of hot climates, that require the stimulus of a great heat to preserve the tone of their less irritable fibres,

are affected by the least abstraction of this habitual stimulus; the irritability of their fibres accumulates in consequence of this abstraction, and the return of the heat again exhausts the fibre. The more intense the cold is, the greater is the accumulation of irritability. After the fibre has been exposed for some time to a great degree of cold, its irritability is increased to such a degree, that the most trifling degree of heat produces the most violent effects: hence the glow experienced in coming out of a cold bath; hence the diseases which are caught in coming out of the cold air into a warm room, and which medical men attribute to checked perspiration, a supposition entirely false.

The least movement is attended with fatigue upon the summit of high mountains, as I have frequently experienced, but especially in 1785, upon the top of the Buet, and, as M. Saussure has likewise observed, upon the summit of Mount Blanc. The reason of it is this; the fibre is rendered so irritable
by

by the cold of these mountains, that the least motion of the muscles, or what is the same thing, the least action of the nervous stimuli, exhausts it. It is by the gradual application only of heat that frozen limbs can be recovered, and it is necessary always to begin by rubbing them with snow; without this the fibre will be exhausted, and become gangrenous. During the winter, by the absence of the stimulus of heat, and in part of light, plants and many animals become torpid, the organs of circulation and of nutrition perform their functions but languidly, and life itself appears suspended. In consequence of the diminished action of these stimuli, the irritability accumulates, and manifests itself at the return of spring. The least degree of heat then produces the most violent effects upon the fibres thus delicately irritable. Animals, which had concealed themselves under ground, venture forth from their subterraneous retreats, plants put forth their leaves and flowers, and man himself is sensible of the stimulus of heat in the gales of spring,

his fibre being rendered more irritable by the winter's cold. Vegetation is much more vigorous in spring time than during the rest of the year. It diminishes during summer in proportion as the irritability accumulated during winter is diminished by the action of heat and light, and, lastly, is exhausted in the autumn. Dr. Hales observed, that the rapidity with which the sap circulates in the vine during spring is five times greater than the rapidity with which the blood flows in the arteries of a horse. This motion is much slower in summer, and almost ceases in autumn. It is not the effect of the heat alone, for if that were the case it would increase as the heat increased, and the effect would be proportionate to the cause; it is the effect of the irritability accumulated in consequence of the absence of heat during the winter. The effects of winter are very great in cold climates, because the accumulation of the irritability is in proportion to the abstraction of the stimulus of heat. In Lapland corn ripens in sixty days, whereas in France it requires

an hundred and twenty, or an hundred and thirty days. The truth of what is here advanced may be proved by exposing vegetables alternately to heat and cold: it is surprizing how much their growth and the power of vegetation is increased. But in these experiments care must be taken to vary the temperature by degrees; because the irritability accumulating in the fibre by the abstraction of the heat, a very small quantity of this stimulus then applied is sufficient to exhaust it entirely, or to destroy it. Hence it is that the return of cold and frost in the beginning of spring is so noxious to vegetables, and that the year is in general more abundant after a very cold winter. Mr. Fontana observed, that during winter the vipers which he kept for his experiments were in a torpid state, though the thermometer was at 59° . He endeavoured to render them vigorous by warmth, and exposed them to a heat of 67° only. In two minutes they died, though during summer they bear a much greater degree of heat; but then they are less irritable. Spallanzani observed that

that newts bury themselves in the earth, and become torpid, in the month of October, before the thermometer in the shade falls to $54\frac{1}{2}$, and that they re-appear in the month of February, though at that time it freezes every night, and frequently during the day the thermometer is many degrees below 54° . What is the reason, enquires this excellent observer, that these animals revive in spring, when the cold is more intense, and sink into torpidity at a much less degree of cold in autumn? I will solve this problem, by observing that in autumn a very great stimulus is required to act upon the fibre of these animals, exhausted as it has been by the heat of the summer; but in spring, the least stimulus, the least increase of heat, is sufficient to put the fibre in action, its irritability having accumulated during winter in consequence of the absence of the common stimuli.

Light is another common stimulus. To convince myself of the stimulating quality of light upon plants by direct experiments, I
enveloped

enveloped the leaves of some plants in an opaque body, so that the air might have free access, while the light could not penetrate. I found that these leaves became more irritable than the others, the irritability having accumulated. By the abstraction of the stimulus of light, the irritability of organized bodies accumulates, and a disease ensues, which is called *étiolement*. Animals deprived of light, and living in dark places, lose their colour and become white, as is observable in arctic animals during the long nights in the countries near the pole: I have observed it also in the animals that inhabit the Alps, and which conceal themselves for the greatest part of the year in subterraneous dwellings. *Blanched* plants lose their green colour, and become whitish and sickly. Some poisonous plants lose their noxious qualities, and become agreeable to the taste, merely by the abstraction of the stimulus of light. White animals and plants are very irritable; and it is observed that these animals and plants are not capable of supporting a great quantity of light.

The

The action of the light upon plants has been very well observed by Dr. Ingenhoufz and Mr. Senebier, and the manner in which colours are produced has been explained by M. de la Metherie. It is well known that animals that have been tamed, and especially domestic animals, change their colour by education; but an observation that has perhaps escaped naturalists is, that this change is constantly from dull colours to those that are brighter or less dull. I have often observed, that the change takes place more frequently in dark than in light places. Mice kept in a cage in a dark room have produced white mice.

The third common stimulus *is that of nutriment*. It requires a very small quantity to supply the daily losses; the greatest portion is employed in depriving the stomach, and of course the whole system, of its superfluous irritability. This is proved by what is observed in organized bodies. All animals are more irritable before than after food. Hunger, of which appetite is the
4 least

least degree, is caused by the accumulated irritability of the system. The gastric juice acts upon the fibres of the stomach now become more irritable, and produces the sensation of hunger. Spallanzani has observed that birds of prey do not void indigestible bodies, such as pieces of glass or metal, which they have taken in with their food, before their stomach is empty. These indigestible bodies cannot be voided while the stimulus of the nutriment acts upon the stomach; but as the abstraction of this stimulus gives the irritability of the stomach an opportunity of accumulating, the indigestible bodies very strongly stimulate the fibres of the stomach, make them contract, and by this contraction they are voided. It is possible to do almost wholly without nutriment, by applying from time to time some other stimulus to the stomach, such as tea, coffee, alcohol, opium, and by exhausting by these means the accumulated irritability of that organ. By the entire abstraction of the stimulus of nutriment,

the irritability of the system is prodigiously increased. There are many instances of persons who, not having eaten any thing for many days, have been intoxicated, and killed, in consequence of swallowing, with great greediness, two or three cups of broth. Plants suddenly transplanted from a meagre, into a very rich soil, produce no fruits or seeds, and die in a short time of a particular disease, caused by excess of nutriment.

The *circulation of the fluids* is the most powerful of the common stimuli. The blood, which oxygenates itself during its passage through the lungs, parts with its oxygene in the circulation, the oxygene having a stronger attraction for the irritable fibre than for the carbon which is contained in the blood. In this operation the heat combined with the oxygene is set free. Hence animal and vegetable heat. The blood acts continually upon the irritable fibre, and the fibre re-acts upon the blood, and this action and re-action are stronger in proportion

proportion as the circulation is more rapid, and as the air which comes in contact with the blood in the lungs contains more oxygenic air.

When any local stimulus continues to act upon any part of the system, the circulation becomes more rapid, and a fever is the consequence. Is the stimulus weak, a slow fever ensues, which will by little and little exhaust the irritability of the system; and the patient will die of a consumption. Is the stimulus stronger, or the fibre upon which it acts more irritable, we shall have an ardent fever, which will exhaust the irritability in a less time. In fine, is the stimulus very violent, or the fibre diseased by an excess of irritability, we shall have a putrid fever, which will destroy the patient, whether animal or vegetable, and will exhaust the irritability in a very short time. But whatever be the nature of the fever, the fibre irritated by the stimulus will act upon the blood more than ordinarily,

rily, the re-action of the blood will be increased in proportion, the circulation will be more rapid, the blood will absorb more oxygene, and the whole system will be furcharged. By this means the irritability will be increased, the animal heat augmented, and the effect of the action of the stimulus becoming greater in proportion to the accumulation of irritability, a total exhaustion of the irritability, or the death of the patient, will ensue. There are two methods of preventing the fatal effects of a local stimulus, whose operation upon one part of the system is constant. The first consists in preventing the surcharge of oxygene in the blood, which is accomplished by diminishing the proportion of oxygene gas in the air breathed by the patient, or by diminishing the quantity of blood by phlebotomy. The second method consists in applying stimuli capable of exhausting the irritability in proportion as it accumulates; such as wine, opium, bark, heat, &c. Phlebotomy acts by diminishing
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the quantity of blood, and consequently its operation is attended with this effect, viz. of diminishing the re-action, and restoring to the fibre its tone. I shall here observe by the way that the advice which many physicians have given, to make the patient breathe oxygene gas, is the most pernicious they can give; for the patient always finds himself worse after having breathed this salutary gas, as I have frequently had occasion to observe.

The *nervous stimulus* is the only one which is peculiar to animals. It is this stimulus which is the cause of the voluntary motions, of convulsions, and passions. The passions differ from one another only in stimulating the irritable fibre more or less. Anger and joy are very powerful degrees of the nervous stimulus; content and hope are weak degrees; fear, sorrow, fright, despair, are not absolute degrees of this stimulus, they are only the abstraction of the stimuli of hope, content, and happiness. Anger and
joy

joy act as very powerful stimuli, and exhaust the irritability of the fibre in the same manner as any other stimulus whatever. Content and hope are degrees of the nervous stimulus, necessary to preserve the tone of the fibre. Sorrow and fright are degrees too weak. If they continue to act, the irritability of the fibre accumulates. It is well known that fearful and melancholy persons are oftener affected by the stimulus of contagious diseases than they who are free from fear, and who take the precaution of applying a greater quantity of stimulus than ordinary to their fibres, by taking wine, vinegar, opium, and bark. According to the observations of Mr. Fontana, timid and fearful animals die much sooner of the bite of the viper than courageous or irritated animals. Joy excited by the annunciation of good news to a sorrowful person, and one of course very irritable, has often caused death. The story of the Roman mother is well known, who was bewailing the death of her son, and who
dropt

dropt down dead for joy the moment she saw him enter her room alive.

By the abstraction of many of the common stimuli for any length of time, the irritability of the fibre accumulates so much, that the most trifling stimulus produces the most violent effects, and frequently even instantaneous death. This disease is called the scurvy, concerning the nature of which medical men have formed so many false and ridiculous theories. It is of the utmost importance to mankind to know the true nature of this disease ; since, in consequence of our ignorance in this particular, we have been unable to find a sure remedy for it, and so many thousands of lives have fallen a sacrifice to its ravages, in armies, fleets, and besieged towns. In the last war the English fleet suffered dreadfully from the scurvy ; and last year a great number of soldiers died of this disease in the imperial army in Walachia, in consequence of the abstraction of the stimulus of nutriment (the emperor having

having ordered that a kind of paste made of bread and water should be given to the soldiers instead of meat), of the stimulus of oxygene, in the corrupted atmosphere of the fens of Walachia, and lastly, of the nervous stimulus, the most powerful of all ; for the greatest part of the army were engaged by force, and against their wills.

The abstraction of all these stimuli accumulated the irritability of the fibre, and caused the scurvy, and that dreadful mortality that took place in the army. The same causes produce the same effects upon animals. We see domestic animals affected with the scurvy in consequence of cold and hunger, that is to say, in consequence of the abstraction of the stimuli of heat and nutriment.

The sheep which captain Cook had on board his ship, on his voyage round the world in the years 1772, 3, and 4, died of the scurvy, their teeth fell out, their gums
2 rotted ;

rotted ; in a word, they had all the symptoms of an inveterate scurvy. The abstraction of the common stimuli in plants produces similar symptoms and a similar disease. The disease of rye called *ergot* is exactly analogous to the scurvy in animals ; the *ergot* is the scurvy of plants ; it is the effect of accumulated irritability in the fibres of plants. The causes which produce the ergot of rye are the same as those which produce the scurvy in animals. According to the observations of Saillant and Teflier, these causes are, a wet and barren soil, and a cold summer ; that is to say, the causes of the ergot are, the abstraction of the stimuli of nutriment and heat. I could enlarge upon this interesting subject, if I were not afraid of making this essay too long. I wished to give only the outlines, or a general view of my theory, without entering into the detail. In the subsequent essays I shall treat of oxygene considered as the principle of irritability, of the composition and decomposition of water in

animals and plants, of the different kinds of air contained in the interior cavities of organized bodies, and of the circulation of this air, the existence of which has not hitherto been even supposed, although, as I shall prove hereafter, the lymphatics in animals, and the fibres in plants, are almost solely destined for the circulation of these elastic fluids.

M E M O I R II.

HAVING given this general sketch of a new system of physiology, founded upon a number of experiments shewing that irritability is the principle of life, I proceed to prove that oxygene is the principle of irritability ; that it unites with the blood in the lungs during respiration ; that it is distributed to every part of the system by the circulation, and that it combines with stimulating substances, with which the different parts of the system come in contact.

I think that the oxygene is absorbed by the blood, and that the venous blood is oxygenated in the lungs during respiration. The most celebrated naturalists and chymists are of a different opinion ; they think that the oxygene does not combine with the venous blood. According to them

this last loses carbone and hydrogene, and recovers the bright colour natural to it, without absorbing any thing from the atmosphere.

Here are some experiments and reasons, upon which their theory of respiration is founded.

1. Arterial blood exposed to the contact of hydrogene air loses its vermilion colour, and assumes the black and deep appearance of venous blood. The hydrogene air is absorbed in part in this experiment.

2. Mr. Hamilton made three ligatures in the jugular vein of a cat. Having expelled the blood from between two of the ligatures, he then introduced the hydrogene air, and kept it there, closing the aperture through which he had introduced it. He then loosened the middle ligature, and the blood contained between that and the third ligature came into contact with the hydrogene

gene air. In about an hour, having taken the blood from the vein, he found it liquid, and of a colour almost as black as ink.

3. Venous blood exposed to vital air acquires the vermilion colour of arterial blood, and the air is rendered impure.

Mr. Lavoisier and Dr. Crawford (*Annales de Chimie*, tom. V. p. 267) have drawn the following conclusions from these experiments :

1. That the change of colour which the blood undergoes during circulation proceeds from its combination with hydrogen air.

2. That in passing through the lungs, the blood parts with a portion of the hydrogen it contains, and then re-assumes its vermilion colour.

Mr. Lavoisier and Dr. Crawford think, that during respiration the vital air which
is

is received into the lungs combines with the carbone and hydrogene that is disengaged from the blood; that it forms carbonic air with carbon, and water with hydrogene; and that the blood recovers its vermilion colour after it has lost the carbon and hydrogene with which it had been charged during the circulation.

Without presuming to contradict philosophers of such distinguished merit, I cannot help observing, that it appears to me that these conclusions do not necessarily follow from the experiments, and that they are to be explained in a manner more conformable to the laws established by modern chemistry. I know no experiment which authorizes us to suppose that carbone can unite with oxygene in a temperature of 97° — 99° , or that hydrogene and oxygene air combine and form water in so low a temperature. M. Seguin has attempted to answer this objection, by supposing that the carbon is in a very attenuated state in the blood, and by citing the experiments of M. Ber-

M. Berthollet upon hydrogen air. But this explanation appears to me hypothetical, and no way convincing.

After having a long time attended to the phænomena of respiration, and made many experiments upon this subject, I think it may be concluded, that during respiration one part of the oxygene of the vital air combines with the venous blood, of which it changes the black colour and makes it vermilion*; the second part of the oxygene unites with the carbon contained in the carbonic-hydrogene gas, which exhales from the venous blood, and forms carbonic acid air; a third part of the oxygene unites with the carbon of the mucus, contained in great quantities in the lungs, and which is continually decomposing; this part also forms carbonic acid air; a fourth part of the oxygene combines with the hydrogen of the blood to form water, which is exhaled during respiration. The

* Dr. Goodwyn had proved this before. Could Dr. Girtanner be unacquainted with his experiments?

heat contained in the vital air being decomposed, remains united in part with the oxygene and the blood. Hence the quantity of heat peculiar to the arterial blood, which is much greater than that of the venous blood. Another part of the heat enters into combination with the carbonic acid air. Lastly, a third part produces a temperature necessary for the formation of water, by the combination of the hydrogen and oxygene airs.

The effects of respiration will consequently be these :

1. The venous blood loses the carbonic-hydrogene air which it contains, and absorbs the oxygene air, which gives it its vermilion colour, such as it gives to metallic oxids, nitrous acids, and many other substances with which it enters into combination.

2. The capacity of the blood will be increased, because oxygene increases the

capacity of all substances to which it unites.

3. The oxygene air of the atmosphere is partly absorbed by the venous blood, changed in part into carbonic acid air by the carbone of the blood and that of the mucus of the lungs, while the rest forms water with the hydrogene air of the blood, and a quantity of heat is set free.

The products of respiration will be—

1. Animal oxid, fluid (i. e. arterial blood.)
2. Carbonic acid air.
3. Water.
4. A small quantity of liberated heat.

Nothing is more easy than to explain by this theory the experiments above related.

If we expose, under a vessel filled with hydrogene air, arterial blood to the contact of this air, the quantity of air will be diminished, lose its vermilion colour, and become livid. In this experiment exactly the contrary takes place of that which is observable in respiration. The hydrogene air unites with the oxygene of the arterial blood to form water, and the arterial blood, being deprived of its oxygene, becomes black, and is changed into venous blood ; the deep colour which it assumes proceeds from the loss of its oxygene alone. The experiment of Mr. Hamilton proves this. He adds, that he found the blood liquid and very little coagulable. This is another proof in my favour. I have said, in my former essay, that the coagulability of fluids obeys the same laws, and depends upon the same principle as the irritability of the solids ; consequently, the blood deprived of the irritable principle, or of oxygene, ought to be liquid, that is, to possess little or no coagulability.

The third experiment is a direct proof
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that the vermilion colour of the blood is owing to the absorption of oxygene.

Having shewn that the arguments upon which the generally received theory of respiration is founded are not conclusive, I proceed to state some direct experiments in favour of the new theory which I wish to establish.

A. Experiments upon venous blood.

1. Six ounces of black venous blood, taken from the jugular vein of a sheep, were introduced into a vessel filled with oxygene air, and in an instant the blood assumed a vermilion colour; the thermometer within the vessel rose several degrees, but sunk again immediately. The mercury in which the vessel was placed rose from six to eight lines. When the experiment was finished, the blood was increased a little in weight; but though I am certain of this increase of weight from repeated experiments, I cannot

exactly ascertain how much it was, because the instruments I made use of for this purpose were not sufficiently exact for so delicate an experiment. The oxygene air which the vessel contained was mixed with carbonic acid air, which lime water absorbed. Some drops of water were formed at the bottom of the vessel.

This experiment proves, that during respiration the blood absorbs the oxygene; and I make no doubt but it is possible to determine the weight of the oxygene absorbed, by repeating this experiment with instruments as exact as those of M. Lavoisier.

This experiment also proves, that during respiration there is formed carbonic acid air and water, that is to say, that there is an exhalation of the hydrogen air from the blood*.

2. The jugular vein of a sheep was opened, and the blood which flowed from

* Rather of the base of hydrogen air.

it was received in a glass bottle filled with oxygene air. The bottle, when half full, was closed. The blood which it contained immediately assumed a vermilion colour, became very fluid, and coagulated but slowly into a reddish and thick mass, without any separation of serum. On the morrow the bottle was opened in order to examine the air which it contained, and the oxygene air was mixed with carbonic acid air. Some drops of water were formed near the mouth of the bottle.

This experiment confirms the first.

3. A considerable quantity of very pure oxygene air was injected into the jugular vein of a dog. The animal raised most terrible outcries, breathed very quickly, and with the utmost difficulty ; by little and little his limbs became hard and stiff, he fell asleep, and died in less than three minutes. Upon opening the thorax and the pericardium, the heart was found more irritable than ordinary, and its alternate contractions and dilatations

lations continued upwards of an hour. The right auricle of the heart was vermilion, and it contained, as well as the right ventricle, a great quantity of blood of a bright vermilion colour, frothy and not coagulated. The blood contained in the left ventricle, in the aorta, and the arteries, was of a rose colour, and was mixed with bubbles of air. All the muscles were more irritable than ordinary. After the blood contained in the heart and veins was discharged, the irritability of the heart and the muscles sensibly diminished.

This experiment appears to me to prove most decisively, that the vermilion colour which the blood assumes in passing through the lungs is not owing to the loss of the carbonic-hydrogene air, but that it proceeds from the combination of the blood with the oxygene air. In the experiment I have described, the livid colour of the venous blood in the right auricle and right ventricle of the heart was changed to vermilion. Nevertheless it could not have lost
any

any carbonic-hydrogene air ; it only acquired oxygene. Besides, this experiment is a direct proof that oxygene is the principle of irritability ; for by furcharging the blood with oxygene, by hyper-oxygenating it, if I may use the expression, the irritability of the blood was, as we have seen, considerably increased.

4. A small quantity of azotic air, which had been exposed for some time to the contact of lime water, in order to separate any carbonic acid air it might contain, was injected into the jugular vein of a dog. The animal died in twenty seconds. Upon opening the thorax, the pericardium, and the heart, the right auricle and ventricle were filled with black thick and coagulated blood. The left ventricle was of its ordinary colour. The heart, and almost all the muscles, lost their irritability almost entirely ; they contracted but weakly upon the application of the strongest stimuli, such as sulphuric æther and the electric spark.

5. The venous blood of a sheep was received in a bottle filled with azotic air. The blood coagulated in an instant, and assumed a colour black as ink. There was a separation of a great quantity of serum. The next day, on opening the bottle, a faint smell of ammoniac was perceivable. The air was azotic air, which extinguished a light.

In this experiment, the azotic air in contact with the venous blood rendered its colour deeper, and even quite black. The ammoniac produced is owing to the hydrogenic air which escaped from the venous blood, and united itself to the azote.

The colour of the blood becoming deeper after it had lost part of the hydrogenic united with it, seems to prove that this deep colour is owing to the carbon of the blood, and not to the combination of hydrogenic air, as has been supposed.

6. A bottle full of carbonic acid air was

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half

half filled with the venous blood of a sheep. It coagulated in an instant, assumed a very deep colour, and there was the separation of a great quantity of reddish serum.

7. A small quantity of carbonic acid gas was injected into the jugular vein of a dog. The animal became sleepy, and died in about a quarter of an hour. The right auricle and ventricle of the heart were filled with thick blood, and in part coagulated. The blood contained in the left ventricle and auricle was of a deeper colour than ordinary. The heart and muscles had lost all their irritability.

This experiment proves, moreover, that the deep colour of the venous blood is not owing to the combination of hydrogenic air. In this experiment, part of the oxygen of the carbonic acid air probably unites itself with the hydrogen of the blood, and forms water, and the carbon, which before was combined with this oxygen, unites with the blood, and gives it its deep colour.

8. An incision was made in the jugular vein of a sheep, and the blood which came from it was received in a bottle full of nitrous air. When the bottle was half filled, it was closed. The blood coagulated immediately, and a separation of a great quantity of blackish serum took place. The day after, on opening the bottle, a very strong smell of nitrous æther (dulcified spirit of nitre) was perceived; the nitrous air having been changed in part to nitrous æther by the carbonic-hydrogene air of venous blood.

This experiment proves beyond a doubt that the venous blood contains carbonic-hydrogene air, and that this air is not very intimately united with it, but that it separates with the greatest ease. The nitrous æther produced in this experiment is owing to the union of the carbonic-hydrogene air, which exhales from the blood, with the nitrous air. The blood, after it has lost this air, does not assume a vermilion colour; but, on the contrary, it takes a very deep colour; it is not, therefore, to the union of the

the

the blood with carbonic-hydrogene, that the deep colour of the venous blood is owing, since this colour becomes still deeper when the hydrogene is separated from the blood.

9. A small quantity of nitrous air was injected into the jugular vein of a dog. The animal died in less than six minutes. The right auricle and ventricle of the heart were filled with blood, thick, black, and partly coagulated. The blood contained in the left ventricle of the heart, was of a much deeper colour than ordinary; the heart had lost its irritability. The lungs were of a greenish cast, and partly putrefied*. All the canal of the wind pipe was filled with a green foam, that came in great quantities out of the mouth of the animal during the convulsions that preceded its death.

Experiments upon arterial blood.

10. An incision was made in the carotid

* The green colour is a sign of nitrous acid, not of putrefaction. B.

artery of a sheep, and the blood that issued thence was received into a bottle full of oxygene air. The bottle, when half filled, was closed. The colour of the blood became in an instant of a bright vermilion. The next day the bottle was opened, and the oxygene gas which it contained was found mixed with a very small quantity of carbonic acid gas.

11. The arterial blood of the carotid artery of a sheep was received into a bottle full of azotic air. The bottle, being half filled, was closed; the blood coagulated at the same moment, and assumed a very deep colour. On opening the bottle the next day, the azotic air which it contained was found mixed with a small quantity of oxygene air, so that a candle burnt in it for near two minutes.

This experiment proves decisively,

1. That arterial blood contains oxygene air.

2. The

2. That it is to its combination with this air that its vermilion colour is owing; and that it assumes its deep colour as soon as it is deprived of its oxygene air.

12. Three ounces of vermilion blood, from the carotid artery of a sheep, were received upon a plate, which was immediately placed under a vessel filled with carbonic acid air. The blood did not change its colour, but continued the same for some hours.

13. Arterial blood, from the carotid artery of a sheep, was received into a bottle filled with carbonic acid air. No change in the vermilion colour.

These two experiments prove that carbonic acid air has no action upon arterial blood, although it has a very great one upon venous blood.

14. The arterial blood of the carotid artery of a sheep was received into a bottle
full

full of nitrous air. The bottle, when half filled, was closed. The blood contained in it coagulated immediately, and assumed a green colour upon the surface. A small quantity of greenish serum was separated. The day after, on opening the bottle, the vapours of nitrous acid were observed by all who were present.

Here then is an experiment which proves, in a most decisive manner, the presence of oxygene in the arterial blood; since it is from this circumstance alone that it is capable of changing nitrous air into nitrous acid. The green colour, observed in this and the 9th experiment, arises from part of the azote separating itself from the nitrous air.

15. Arterial blood, from the carotid artery of a sheep, was received into a bottle full of hydrogen air, which was closed when half filled. The blood became of a brighter vermilion, and remained fluid for some time. It coagulated at last, and a small quantity of
serum

serum was separated. The day after, the hydrogen air contained in the bottle was found mixed with a small quantity of oxygen air, which the nitrous air absorbed. This experiment proves the presence of oxygen air in arterial blood.

16. The arterial blood, as before, was received in a bottle containing equal portions of oxygen and hydrogen air. The bottle, when half filled, was closed. The blood in the bottle became moderately hot, remained fluid, and was of a more vermilion colour. It coagulated at last, and a small quantity of serum was separated. The day after, the air in the bottle was mixed with a small quantity of carbonic acid air, of which the presence was ascertained by lime water.

17. A small glass tube was filled with arterial blood of a bright vermilion; it was sealed hermetically, and exposed to the light. The blood changed its colour
by

by degrees, and in six days became as black as venous blood.

18. The same experiment was repeated, with this difference only, that the tube was exposed to heat, and not to the light. The blood became black in a shorter time.

The 17th and 18th experiments made by Dr. Priestley, and repeated afterwards, appear to me to demonstrate, that it is not to the contact with hydrogen air that the venous blood is of a black colour.

I conclude from these experiments,

1. That the change of colour the blood undergoes during circulation is not owing to its combination with hydrogen air.

2. That the deep colour of the venous blood, is owing to the carbon it contains.

3. That the vermilion colour of the arterial

terial blood proceeds from the oxygene with which the blood is combined, during its passage through the lungs.

4. That respiration is a process exactly analogous to the combustion and oxidation of metals; that these phænomena are the same, and to be explained in the same manner. .

5. That during circulation, the blood loses its oxygene, and charges itself with carbonic-hydrogene air, by means of a double affinity.

6. That during the distribution of the oxygene through the system, the heat which was united with this oxygene escapes; hence the animal heat.

7. That the great capacity of the arterial blood for heat is owing to the oxygene with which it is united in the lungs.

Having shewn that the blood is oxygenated in its passage through the lungs; that

in the circulation it loses the oxygene it had absorbed ; and that it returns to the lungs furcharged with carbonic-hydrogene air, it remains to prove, that to this oxygene, distributed through every part of the system, is owing the irritability and the life of organized bodies. Here follow the proofs upon which this theory is founded.

The irritability of organized bodies is always in a direct ratio of the quantity of oxygene they contain.

1. Every thing that increases the quantity of oxygene in organized bodies, increases at the same time their irritability.

We have seen a direct proof of this in the 3d experiment, cited above. Besides this, a great number of other phænomena support my opinion. The irritability of animals made to breathe oxygene air is wonderfully encreased. Blanched plants, whose irritability has been accumulated in
consequence

consequence of the abstraction of the stimulus of light, contain a great quantity of oxygene, according to the experiments of Mr. Fourcroy. I have observed, in the course of my experiments, that plants made to grow in oxygene air become white, although exposed to the light. But what shows more clearly than all, that the irritability is always in proportion to the quantity of oxygene, are the phænomena attending the action of mercury and mercurial salts upon animals. As this is one of the most striking proofs of my theory, and as I have before observed, that many persons, and amongst the rest philosophers of the first rank, such as Dr. Crawford, have been struck with the novelty and simplicity of my mode of explaining these phænomena, I cannot forbear entering into some detail upon this subject. It is a well-known fact amongst physicians, that mercury, in its metallic state, has no effect upon the human body. I have known many people, who for many years took a daily portion of quicksilver, to the amount of one or two ounces, from an

idea of guarding themselves from epidemic diseases, but who never perceived any effect whatever from this singular custom. It is proved also by the experiments of Dr. Saunders, that the effects of mercurial ointment are owing only to the small quantity of mercury that has been oxidated during a long trituration. It is necessary, therefore, that mercury should be oxidated, to have any effect upon the human body. On the other hand it is well known, that in persons who have rubbed themselves with mercurial ointment, or who have taken the oxid of mercury, the mercury, after having produced its usual effects, has passed through the skin in a metallic form, and has amalgamated itself with watches, and the gold in the pocket, &c. The oxid of mercury, in passing through the human body, parts with its oxygene, and it is to this oxygene alone, which remains combined with the system, that the effect produced by oxidated mercury is owing. This effect is the mercurial disease, the symptoms of which are the same as those of the scurvy; the mouth,

mouth, gums, and the whole system are affected in a manner extremely analogous. But the scurvy, as I have proved in my first essay, is a disease produced by the accumulation of the irritable principle*. The accumulation, therefore, of the oxygene producing the same effects, the great analogy between the irritable principle and oxygene appears to be proved, and I think myself authorized to conclude, that oxygene is the principle of irritability.

(M. Berthollet, in the Paris Memoires, 1780, has attributed the causticity of metallic oxids to the oxygene they contain).

2. Whatever diminishes the quantity of oxygene in organized bodies, diminishes at the same time their irritability.

This has been shewn in the 9th experiment, where the heart and the muscles lost their irritability, having been deprived of their oxygene by nitrous air. But not to

* Certainly erroneous.

leave any doubt upon this subject, I made the following experiment.

Experiment 19. The heart of an animal just killed was cut into pieces, and put into a glass retort, to which was affixed a pneumatic apparatus. A very small degree of heat was applied to it, by means of a lamp placed under the retort. When the pieces were heated, bubbles of air were perceived in the pneumatic apparatus. They remained exposed to the same degree of heat for nearly two hours, till the surface was just burnt. Upon examining the air which had passed into the apparatus, it was found that the first portion of air was the atmospheric air of the retort, mixed with a very small quantity of vital air, whose presence was ascertained by nitrous air. The second was vital air mixed with carbonic acid air.

I have repeated this experiment upon many other parts of animals just killed, and I have always obtained a greater or a less quantity of oxygene air. It is possible to obtain

tain the same quantity of this air many times following, by exposing the animal substances alternately to the atmospheric air, and to a heat of 60 or 70 degrees of Reaumur. I shall observe, however, that these experiments are very difficult to make, and some time is requisite to ascertain the degree of heat necessary to disengage the oxygene air. If the heat applied be too great, carbonic acid air will come over instead of oxygene air. It is possible to extract all the oxygene which animal substances contain, by means of hot water: it is thus we make jellies. These jellies are always more or less transparent, which, without any other proof, would be sufficient to authorize us to suppose the presence of oxygene in jellies, because it is certain that all transparent bodies except alcohol and æther owe their transparency to the oxygene that enters into their composition*. I have proved, that oxygene combines with venous blood in the lungs; that it is distri-

* This is reasoning very rapidly. Who has detected oxygene in rock crystal?

buted to all parts of the system by the circulation ; that to this principle irritability is owing : it remains only to examine what becomes of the great quantity of oxygen which all parts of the system are continually receiving from the blood. I shall attempt to prove, that the different stimulating substances absorb this oxygen.

I have observed in my first essay, that there are three different states of the organized fibre.

1. The state of health or tone of the fibre.

2. The state of accumulation, in which the fibre is surcharged with the irritable principle.

3. The state of exhaustion, in which the fibre fails through want of the irritable principle.

All substances capable of coming in con-

tact with the irritable fibre, can likewise be ranged under three classes, of which

The first comprehends the substances which have the same degree of affinity to the irritable principle, or the oxygene, as the organized fibre itself. These substances produce no effect upon the fibre.

The second contains those which have a less degree of affinity to the oxygene than the fibre has. These substances, coming in contact with the fibre, will surcharge it with oxygene, and produce the state of accumulation. These substances may be called negative stimuli.

The third class contains those substances which have a greater degree of affinity to the oxygene than the fibre itself has. These, coming in contact with the fibre, will deprive it of its oxygene, and produce the state of exhaustion. I shall call these substances positive stimuli.

It is a fact known at this time, that the

affinity of different substances varies considerably according to the degree of temperature. The same variety takes place in the organized fibre. I shall observe, therefore, in order to be exact, that when I speak generally of the affinities of the irritable fibre, I mean always in the ordinary temperature of the blood of warm animals.

I will make some observations upon each of these classes.

The first class comprehends, as I have said, substances having the same degree of affinity to the oxygene as the irritable fibre. All organized, or living substances, are to be ranked under this class. (Note, The words organized and living are, in my opinion, synonymous. I regard as living, every body, each part of any body, in a word, all organized substances, as long as they contain the principle of irritability, or of life, and as long as the affinities are the same as those of living substances. The wood, for instance, of which our chairs and tables are

2 made,

made, is an organized or living substance; and to speak properly, it cannot be said that the wood is dead before it be rotten, and so of the rest. Our ideas of life and death are very vague ideas, and I shall attempt to fix them in some other way). These substances produce no effect upon the irritable fibre, while their degree of temperature is the same as that of the fibre with which they come in contact.

I have arranged, in the third class, the positive stimuli, that is to say, those substances which have a greater degree of affinity to the oxygene than the fibre has. These substances, coming in contact with the fibre, combine with the oxygene it contains, deprive it of its irritability, and leave it in a state of exhaustion. There is a great number of these substances. The most known are alcohol, sulfuric æther, opium, and other narcotics, oil of lauro-cerasus, and oils in general, grease, sugar. All these substances are combustible, that is to say, they have a great affinity to oxy-

2 1 2

gene,

gene, and it is by this property that they deprive the organized fibre of its irritability, by combining with the oxygene it contains. The second class comprehends the negative stimuli, or substances, which have a less affinity to the oxygene than the fibre has. Some of the most terrible poisons we know of must be ranked under this class. The oxygene which combines with the organized fibre, when it comes in contact with these poisons, renders it so extremely irritable, that the weakest stimulus is capable of producing death; by a law of irritability which has been explained in the first essay. Oxygenated marine acid is for this reason so fatal a poison to all organized bodies. It destroys them by furcharging them with irritability, that is, by hyper-oxygenating them, and becomes marine acid by this operation.

Arsenic, under its metallic form, has no effect upon animals; but the white oxid of this metal is one of the most terrible poisons; for it hyper-oxygenates the organized fibre

fibre with which it comes in contact, and re-assumes its metallic form. Oxygenated metallic salts, such as the corrosive or muriatic sublimite of mercury oxygenated, &c. produce the same effects. The oxids of silver and mercury produce greater or less effects upon the organized fibre, in proportion as they contain more or less oxygen. The black oxid of mercury, otherwise called æthiops, produces the most trifling effects; the red oxid of the same metal produces the most terrible effects, and destroys organized bodies in a very short time. The same explanation applies to the action of fulfate of tin and lead, and the acetate of lead and brass upon the organized fibre.

I am convinced by experiments which I shall relate some other time, that the organized fibre, both animal and vegetable, decomposes the water that comes in contact with it. The greatest part of the water we drink is first decomposed, and then recomposed. It is indeed one of the means by which nature furnishes organized bodies with

with the oxygene necessary to preserve their irritability and life. This discovery explains very many phænomena hitherto inexplicable. I have also reason to think, that by this discovery of the decomposition of water by organized bodies, we shall be able to explain the most hidden mysteries of animal physiology. Reflecting upon the results of several of my experiments, I begin to suppose that the hydrogene air, which remains after the oxygene of the water is united to the irritable fibre, may serve to supply the loss of nervous fluid, or, in other words, I suppose that the nervous fluid is the hydrogene air, perhaps carbonic-hydrogene gas. I confess this is only a conjecture, which I am not yet able to prove, but which appears to me very probable. Be this however as it may, it is very certain that water is decomposed and recomposed continually in organized bodies. This is clear from experiments I shall hereafter enumerate.

I have explained the phænomena of hunger in animals; I have said, that this sensation

sensation was the consequence of irritability accumulated in the system; and that for a substance to be nutritious, it must be a positive stimulus; namely, one that has a great tendency to unite itself to the oxygene, because it is only by uniting itself with this principle, with which the system is furcharged, that it can restore the tone of the fibre, and allay the painful sensation of hunger. Every phenomenon supports this theory. Different substances nourish only in proportion to their affinity to oxygene. Living animal substances (oysters for instance) afford little or no nourishment, because they cannot combine with the oxygene, with which they are already saturated; hence the common observation, that oysters increase the appetite. Animal jellies, fruits, vegetable substances in general, afford little or no nourishment. Animal food just killed does not nourish so much as that which has been kept some time; and raw meat is not so nourishing as that which has been cooked. Hence all the art of cookery, which consists only in depriving
the

the food of its oxygene, by applying different stimulating substances, and, above all, the stimulus of heat. Roasting the food is the most simple manner of cooking it; whilst it is exposed to the heat, it parts with its oxygene, as in the 19th experiment. Oils, fat, sugar, alcohol, and other substances, which have a great affinity to oxygene, are very nourishing. In the East Indies, millions of men support themselves by small quantities of opium, when the rice harvest fails them, as very frequently happens in those wretched countries, groaning under the despotism of a company of English merchants.

Thirst is a state of the system opposite to that of hunger; it is a sensation which indicates a state of exhaustion, a deficiency of oxygene. Every thing that restores to the fibre its lost oxygene, puts an end to this disagreeable sensation. Water produces this effect by its decomposition when it comes in contact with the fibre. The same effect will be produced by vegetable acids,
which

which are always decomposed in the stomach of animals. It is only in proportion to the oxygene in the composition of the acids, and to which they have but little affinity, that they refresh and allay the sensation of thirst. Thus vegetable acids are the best remedies against the effects of narcotic poisons; for by their decomposition they restore to the fibre the oxygene which the poisons had deprived it of. Vinegar, taken in large doses, cures the state of exhaustion produced by a strong dose of opium, and prevents death, which would otherwise ensue. It is well known that drunken persons become sober by drinking a glass of vinegar; that is, the vinegar restores the tone of the system which it had lost, by the effect of the alcohol contained in the wine. A great quantity of water produces the same effect.

Many other phænomena may be explained upon the same principle. We find the air fresher and more agreeable after heavy rain, because the watery vapours

which rise from the earth, and come in contact with our bodies, are decomposed and restore the lost oxygene *. The phænomena displayed by the rotifer, that singular insect, which, though entirely dried up, may be revived by moistening it with a drop of water, appear inexplicable; but it seems that it is easy to account for it on my principles. The drop of water is decomposed, the oxygene it contains combines with the rotifer, restores its irritability, its life, and organic motion, of which it had been deprived by the stimulus of heat, to which it had been exposed in becoming dry.

Amongst the known positive stimuli, those which produce the greatest effects are the stimulus of putrid fevers, or of the plague, and that of the mephitis, which exhales from putrefied animal substances in places where the air cannot enter, as in tombs and burial places. The affinity which this mephitic gaz has for oxygene is so great,

* Nonsense. The air is only become a better conductor of heat.

that as soon as it comes in contact with the fibre, it deprives it of its oxygene, and causes death, frequently in an instant. The best way to prevent the fatal effects of this gaz is, to detonate nitre upon burning charcoal. 'The oxygene gaz which escapes during the decomposition of the nitre supplies the oxygene which combines with the mephitic air. This theory is so true, that the workmen who have been suffocated by the mephitic air exhaling from tombs have recovered their senses and been refreshed (according to their own expression) as soon as they have been made to respire oxygene air.

I shall hereafter relate the experiments I have made upon vegetables, with many stimulating substances, but, above all, with alcohol, opium, the solution of white oxid of arsenic, vinegar, water, heat, and the oxids of mercury. I have found that these substances had effects upon plants similar to what they had upon animals; that the irritability of the most irritable plants, such

as the mimosa and hedysarum, may be entirely destroyed by positive stimuli, by opium for example, alcohol, or heat; and that it is possible to give very sensible irritability to plants which did not appear to possess it, by applying, for some time, negative stimuli, such as vinegar, or white oxid of arsenic. I hope that the result of these experiments will be of considerable use to agriculture, by shewing us the true nature of plants, their diseases, and the means of remedying them. I have found that oils and alcohol, employed in small quantities, are specific remedies for the diseases of plants, produced by the accumulation of the irritable principle; diseases marked by the yellow colour which the leaves assume.

IT is easy to supply the latter part of this theory with a necessary correction. It is not credible that many positive stimuli, such, for instance, as the vegetable poisons of the tropical countries, and the venom of certain serpents, which produce death, in quantities so astonishingly minute, should, as Dr. Girtanner

ner imagines, exhaust the irritable principle, by combining with it directly themselves; neither, surely, can alcohol, opium, and oil of lauro-cerasus, be supposed to attract from the irritable fibre, if we consider only the quantity in which they produce their effects, a large quantity of oxygene. Is it not more likely that they occasion, throughout the whole system, a new combination of oxygene? The blood, and the muscles of animals destroyed by positive stimuli, and of those destroyed by negative stimuli, as nitre or arsenic, ought, according to this idea, to exhibit appearances diametrically opposite.

It is also evident, that Dr. Girtanner's, or, more properly speaking, Dr. Brown's, *Materia Medica* is too scanty: negative stimuli will be necessary to correct certain aberrations of the system from health. His idea of negative stimuli, as I often observed to Brown and his disciples in 1785 and 6, supplies one of the greatest defects of Brown's system. It is in vain to say of those substan-

ces which directly, and without any appearance of previous excitement, in whatever quantity they are administered, such as lead, diminish the actions of life, that they are less powerful stimuli; for how, on this principle, can they lessen the effect of the ordinary stimuli, which are applied at the same time? It is equally in vain to say (what Dr. Girtanner repeats), that the depressing passions are only the abstraction of the stimuli of the exciting passions. Universal experience, I apprehend, will immediately reject such a scale of mental affections, as this system supposes; a scale which must present the passions somewhat in this order; supposing the most exciting to stand uppermost:

ANGER,

JOY,

HOPE,

CONTENT,

INDIFFERENCE, { As far as the mind
is capable of indif-
ference.

FEAR,

SORROW, and so on.

Now

Now is it credible or possible, that sorrow should be to indifference what darkness is to twilight? Every individual almost is capable of judging, whether the thoughts of a person in affliction are less busy than of an idiot? whether the mind of one under the influence of the distressing passions is less upon a stretch, than of him, “Who Whistles as he walks for want of thought?”

During such a state of vacancy, it will hardly, I presume, be thought that there is more mental exertion than in the most profound sorrow, even if we admit the common supposition, that in this state the mind rests upon the contemplation of one or a few ideas; a supposition which requires much limitation.

Brown very properly warns his readers of the methaphorical nature of his terms.*

We

* *Partim ob incertam rei naturam, partim ob sermonis ægestatem, item hujus doctrinæ novitatem, incitabilitas modo abundare, cum stimuli parum admotum, modo deficere, exauriri aut consumi, cum is vehementius incubuit,*
possim

We must therefore, without regard to them, if observation indicates it, admit a power in some drugs, in some of the passions, and

passim deinceps dicetur. Tum hic, cum alias, ubique
 REBUS VERIS STANDUM: *lubrica causarum utpote*
ferè incomprehensibilium, questio, venenatus ille philosophiæ
anguis, cum curâ fugienda. Nequis igitur, per modo re-
lata dicta, incitabilitatis naturam respici, aut, an materia
sit, et sic modo augeatur, modo imminuatur: an adhærens
materiæ facultas, nunc vigeat, nunc langueat, definiri:
aut ullo modo reconditam questionem attingi, quod magno
scientiæ malo semper ferè factum, interpretetur. l. c. I. 5. 6.

The warning against the enquiry concerning causes, as being incomprehensible, requires some explanation. Causes are only phænomena, uniformly preceding other phænomena; they are, therefore, just as comprehensible as effects: we cease to comprehend just where we cease to perceive. The bane of philosophy is the transferring of phænomena, by the imagination, from one part of nature to another, where the senses discern no traces of their existence; and placing them as antecedents before observed phænomena. As this imaginary succession will be wrong 999 times at least in 1000, we shall as often fail when we attempt, in practice, to modify the consequents, by regulating the supposed antecedents.—But, by discovering the real succession of phænomena, we shall assuredly gain infinite advantages. Thus, were oxygene the principle of irritability, we should not only

and in some external circumstances, either of preventing the system from giving out its excitability, or from accumulating it (which state is very often seen, where a continuance of sleep, sufficient as to its duration, does not refresh, or, according to Cullen's expression, *render the system more liable to be affected by stimuli of all kinds*), or on the contrary, of accumulating it too rapidly. We are even enabled, by the foregoing hypothesis, to conceive a *modus operandi* in all these cases.

These principles, with which the late wonderful discoveries of Mr. Galvani, Valli, and Volta, seem perfectly well disposed to coalesce, promise all those advantages which would result from a perfect knowledge of the mechanism of the animal functions. Was

only have the satisfaction of comprehending more clearly the actions of the living system, but we should have them more in our power, than while we were ignorant of this truth. While, therefore, we religiously ABIDE BY FACTS, we can do nothing so well as investigate causes.

not Mayow, for instance, infinitely nearer the truth, than any author of a later hypothesis, when he imputed muscular motion to the *effervescence of his nitro-atmospherical particles*? Does not muscular contraction or intumescence really depend upon the combination of oxygene with hydrogene and azote (separately and combined, in various proportions), in consequence of a sort of explosion produced by the nervous electricity? According to this hypothesis, animal motion, at least that of animals analogous to man, would be produced by a very beautiful pneumatic machinery; and our nervous and muscular systems may be considered as a sort of steam-engine: This hypothesis, though not perhaps at this moment capable of strict proof, is extremely probable, since it is countenanced by every observation and experiment yet made on the subject. It accounts for the perpetual necessity of inhaling oxygene, and enables us to trace the changes undergone by this substance, from the moment it is received, till the moment it is expelled. By the blood it is imparted

to the muscular fibres ; here, during their contraction, it combines with the elements above-mentioned into water and various salts, among which the marine and phosphoric acids deserve particular notice. In this state it is taken up by the absorbents, and afterwards exhaled, or excreted. Observations, adduced in the preceding pages, sufficiently shew the necessity of oxygene to muscular motion ; and how the power of motion languishes, when this principle is scantily supplied. It appears that *meat* becomes tender in consequence of the secondary combination of oxygene, in whatever way this secondary combination be effected ; whether by keeping it till the putrefactive process takes place more or less ; by cookery ; by obliging the animals to undergo violent exercise before death, as in hare-hunting, bull-baiting, and in an expedient of gluttony, rather more barbarous than either of the preceding, that of flogging poultry to death. The flesh of animals so destroyed ought to be more succulent, as well as more tender. It is an observation

of experienced sportsmen, that an hunted hare will continue to emit steam, when brought to table, very much longer than an hare otherwise killed. I have heard the same remark made with respect to hunted venison. These phænomena correspond perfectly with the supposition of liquids, partly volatile, being generated during muscular action.

In the West India Islands they kill their poultry with vegetable poison, in order to render them tender without keeping. Stimuli, which are only less violent poisons, are sometimes used for the same purpose in this country. It does not appear whether they produce their effect immediately, or by first exciting the nervous electricity. But whatever be the mode of action of these poisons and stimuli, that of contagious miasmata seems to be exactly the same.*

The

* I once saw an instance, in which I could not doubt that complete intoxication was produced by the contagion of typhus, to which the person had been much exposed. One morning, immediately upon rising, and

I knew

The similarity of the symptoms in typhus and scurvy has been frequently noticed; and the similar symptoms of these diseases seem evidently to depend upon the same cause; the contagion of typhus depriving the system of oxygene, by causing the combination of a great part of that which it already contains. Hence it is probable that the true indication of cure in typhus is to

I knew that he had been perfectly sober the night before, I was astonished to observe that flighty vivacity and disposition to wild disjointed talk, together with the other signs that infallibly denote a certain degree of intoxication, especially when you are well acquainted before-hand with the manners of the party. In the course of the day, during which I saw him frequently, he became heavy, had febrile shiverings, and complained of head-ache. The next day he became more feverish, but was not confined till the fifth day, though the head-ache and other symptoms never quitted him. He passed through all the stages of typhus, but never seemed to be in imminent danger. In most instances, the period of the excitement of the brain is not perceived; we, however, frequently see the action of the vascular system increased at the onset of typhus; this increase of action sometimes misleads practitioners into the fatal measure of blood-letting.—Does not the highly saline urine in febrile diseases, and after exercise, depend on the chemical combinations above-mentioned?

restore the oxygene; and it is likely that upon this principle, a certain and speedy cure will be contrived. The modern practice, which employs stimulants very freely, though, upon the whole, I believe, not so mischievous as the contrary, is not such as we ought by any means to acquiesce in. It does not ensure so much success as might be expected from a method founded on just principles; and, indeed, as far as I can learn, the different methods in use answer pretty much alike, and the disease is very little in the doctor's power—Oxygene may be more beneficial at one stage of typhus than at another.

Those cases where typhus attacks persons after exposure to severe cold, at a time when you cannot, by the strictest scrutiny, discover any previous vestiges of contagion in the neighbourhood, render it highly probable that this disease may be produced by ordinary stimuli applied to excitability much accumulated. The symptoms of the influenza, which are scarce distinguishable from those of catarrh, as well as the effect produced by the steward's visit to the natives

tives of St. Kilda, (if this respectably attested though surprising, narrative be true) afford another instance where diseases, extremely similar, at least, are produced by ordinary stimuli, and by the extraordinary stimulus of contagion. If the marsh-miasma be not an imaginary being, there is reason to presume the same thing of intermittents, which very often appear where marsh-miasma cannot well be supposed present.

One may, I conceive, reasonably expect to remove the feverishness, or indirect debility, that follows intoxication, by causing the person who is suffering under it to respire oxygene air. This would, perhaps, not only make up the waste of this principle, but also restore the nervous electricity; a circumstance to which it will always be necessary to attend in disorders of excitement, or produced by excitement. From the experiments of Mr. Saussure and Volta, on the electrical phænomena attending condensation, it may be conjectured that the
animal

animal electricity is renewed by respiration. The want of some certain method of effecting this will, perhaps, produce some doubt in the reader's mind respecting the certain efficacy of an hyper-oxygenated atmosphere in typhus; but the few imperfect trials that have hitherto been made upon the respiration of oxygene air, seem to me no more than counterbalance this doubt. The experiments of Mr. Fourcroy, the best upon record, seem to promise the happiest success.

Attention is undoubtedly not less due to the other elements of organized bodies; and if the importance of oxygene seems to have been magnified in the foregoing observations, it is only because we have few or no facts which afford a foundation for reasoning concerning the connection of an excess or deficiency of hydrogene or azote with the functions of life: and yet much obscurity and many difficulties must be expected to remain, till we acquire the knowledge of such facts. This reflection should render

us only the more attentive to the phenomena of life; for if we can but perceive enough to suggest a new hypothesis, capable of being verified by experiments, physiology will not fail to gain something, and perhaps something considerable, even by the proof of its falsehood. This reflection should also teach us to set a due value on our present knowledge, though it be imperfect; and it should restrain those rude hands that are ever ready to pluck up the tender plants of science, because they do not bear ripe fruit at a season when they can only be putting forth their blossoms.

A boundless region of discovery seems to be opening before us. Physical science, which began with remote objects, now promises to unfold to us the more difficult and more interesting knowledge of ourselves. This kind of knowledge will assuredly, as Dr. Girtanner observes, become a part, and the most important part, of education; and it will effect a greater improvement in the morals of mankind, than all the

sermons that ever have been, or ever will be preached. Physiological ignorance is the most abundant source of our sufferings ; every person accustomed to the sick, must have heard them deplore their ignorance of the necessary consequences of those practices, by which their health has been destroyed ; and when men shall be deeply convinced, that the eternal laws of nature have connected pain and decrepitude with one mode of life, and health and vigour with another, they will avoid the former, and adhere to the latter. And as actions are named immoral from their effects, self-love and morality are so far perfectly the same. Nor is this sort of morality likely to terminate in itself ; but the habit of acting with consideration, and upon principle, will extend from the selfish to the social actions, and regulate the whole of life.

If it be said, that the general rules of health have long been deduced from universal experience, it may be answered, that the difference between the perception of two events,

events, which, though seemingly allied, are placed at a great distance from each other, with a blank between them, and a distinct view of the whole succession of operations that inseparably connect them, is immense. In the former case, there will seem to be room for the caprice of chance to intervene and separate them; in the second, there cannot be room for any such delusive expectation; and this more particular information will render it much more difficult for a person to impose upon his own understanding, than it has hitherto been.

It has always, indeed, appeared to me an essential requisite in a tolerably constituted seminary of knowledge, to provide the means of popular information on the means of preserving health; but peculiar advantage may now be expected from such an institution. Its extreme imperfection has hitherto rendered medicine a particular craft, little worth studying, but for the money it would bring. A medley of error, non-

sense, and contradiction, was not like'y 'o engage volunteer students ; for nothing is more intolerable to most minds, than to lie tossing upon a sea of doubt. At present there is beginning to appear, in physiology and pathology, something like the simplicity and certainty of truth. In proportion as the laws of animal nature come to be ascertained, the study will be gradually esteemed more worthy of general attention, and in spite of the disgust raised by anatomy at first, it will finally prove the most popular, as being the most curious and interesting, branch of philosophy ; and a *New Medicine* will at length arise, with healing on her wings, from the ashes of the *Old*

ADDENDA.

A D D E N D A.

THE following is the passage of the *Flora Lapponica* alluded to at page 58. It were to be wished that the few pages of incidental observations, occurring in that work, were printed separately. In spite of much quaintness of language and puerility of sentiment, they would interest many readers, not likely to meet with them in their present situation :—

O felix Lappo! qui in ultimo angulo mundi sic bene lates contentus et innocens. Tu nec times annonæ charitatem, nec Martis prælia, quæ ad oras tuas pervenire nequeunt, sed florentissimas Europæ provincias et urbes, unico momento, sæpe dejiciunt, delent. Tu dormis hic sub tuâ pelle ab omnibus curis, contentionibus, rixis liber, ignorans quid sit invidia. Tu nulla nosti nisi tonantis Jovis fulmina. Tu ducis innocentissimos tuos annos ultra centenarium numerum cuni facili senectute et summâ sanitate. Te latent myriades morborum nobis Europæis communes. Tu vivis in sylvis, avis instar, nec sementem facis, nec metis, tamen alit te Deus optimus optime. Tua ornamenta sunt tremula arborum folia, graminosque luci. Tuus potus aqua crystallinæ pelluciditatis, quæ nec cerebrum insaniam adficit, nec strumas in Alpibus tuis producit. Cibum tuum est vel verno tempore piscis recens, vel æstivo serum lactis, vel autumnali tetrao, vel hiemali, caro recens rangiferina absque sale et pane, singulâ vice unico constans ferculo, edis, dum securus s lecto surgis, dumque

dumque eum petis, nec nosti venena nostra, quæ latent sub dulci melli. Te non obruit scorbutus, nec febris intermittens, nec obesitas, nec podagra, fibroso gaudēs corpore et alacri, animoque libero. O sancta innocentia, estne hic tuus thronus inter Faunos in summo septentrione, inque vilissimâ habitâ terrâ? numne sic præfers stragula hæc betulina mollibus serico tectis plumis? Sic etiam credidere veteres, nec male.

HAPPENING, in a company where the Reverend Mr. Leslie was present, to mention my opinion of the possible good effects of air, containing less oxygen than common, in consumption, he related some circumstances relative to the academy at Liege, which he thought gave some countenance to my idea; and afterwards favoured me, at my request, with the following particulars:—

Cum a me, vir eruditissime, postules ut chartæ ea commendem quæ nuper in familiari colloquio asserui circa modum procedendi cum adolescentibus in academiâ Anglorum Leodii in Germaniâ, cum morbo vulgo dicto consumptione vel feбри hectica laborarent, hoc eo lubentius facio, quod exindè ope principiorum artis medicæ forsitan poteris aliqua deducere quæ humano generi plurimum proderunt.

Notandam imprimis academiâ illam, in quâ ego ipse per plures annos habitavi, in vertice alti montis prope arcem civitatis Leodiensis esse sitam, et aerem ibi esse tam purum ut in eo moniales Anglæ, quæ propé academiâ

miam

miam olim habitabant, ut plurimum pulmonum consumptione vitam amiserint, et ob illam causam in infimâ urbis parte, prope Mosam flumen, in denso aere domicilium fixerint, ubi raro consumptione laborant. Exindé partim et ex aliis observationibus mos invaluit in academiâ, adolescentes consumptione laborantes ad loca infima, nebulosa, et paludosa Flandriæ Austriacæ mittere, Brugas puta. Antwerpiam, vel Gandavum, ubi ope densioris aeris intrâ paucos menses sanitatem fermè semper recuperabant; si verò ad academiam redibant, iterum in eundem morbum incidebant. Si cui verò in mentem venerit dubitare an consumptione propriè dictâ laboraverint, ex sequentibus symptomatibus sive indiciis poterit ferre iudicium. Laborabant initio morbi tussi fermè continuâ, deinde sputa sanguinolenta mittebant, tum purulenta et fœtida, macilentiores quotidie evadebant, pallidus illis erat ut plurimum vultus, sed interdum subito roseo colore diffusus, oculi ut plurimum viridi et acuti.

Hæc pauca currente calamo, sine terminorum technicorum ornatû conscripsi; sed si crudus hic scribendi modus displiceat, nihil vetat quominus possis ea, non mutato sensû, in meliorem formam redigere. Vive, vale, vir doctissime, et epistolâ hâc, siquid prodesse possit, rutere.

C. LESLIE.

Oxonii, die 7^o Sept. anni 1792.

THE following *conjecture concerning the use and effect of manure*, is part of a paper read before the Chemical Society at Edinburgh, April 7, 1786, and printed in that very excellent miscellany, the Edinburgh Magazine, for one of the two following months. The theory advanced in it still appears to me, so far as it goes, to be just, and as it is so much akin to the speculations contained in the preceding pages, I have subjoined it here.—

The result of Dr. Ingenhousz's experiments on vegetables exposed to the light of the sun is well known. Since the publication of his English work, he has been more or less constantly employed on the same subject; and on occasion of some controversies, has published both in French and German many experiments, all tending to the same conclusion. His chief controversy was with Mr. Senebier of Geneva, which, however, has terminated completely in his favour, for his antagonist has publicly acknowledged that he was totally misled by some inattention in conducting his processes. I find too in the *Acta Theodoro-Palatina* (Vol. V. 1784,) a very long series of experiments by Professor Succow of Mannheim, which exactly coincide with those of Dr. Ingenhousz.

He concludes his account of them in the following manner. ‘Those effects of the solar light on plants, which Dr. Ingenhousz first so admirably pointed out, are confirmed by the preceding experiments, in which trees and plants appeared most capable of yielding pure air in the light of the sun; whereas in the shade they afforded air more or less phlogisticated. That the air which is extricated
when

when vegetables are exposed in water to the sunshine, proceeds from their leaves and other parts, scarce needs any proof. Water indeed does contain a quantity of air which is disengaged by the influence of light; but the quantity is so sparing even in a large quantity of water, that it can by no means be set in competition with that which vegetables yield in the course of a few hours. Did this air proceed from the water, it would in very few cases prove so pure, unless the water contained some of the green conferva; but then it would be to this moss that the origin of the air must be ascribed. 'The difference in the air itself which vegetables yield when other circumstances are alike, puts it beyond all doubt, that the air must proceed, not from the water, but the vegetables.' I shall leave the society to judge how many negative results will be requisite to destroy the force of these numerous experiments, and proceed to what I have more particularly in view.

Dr. Ingenhousz, in his last work, (*Verm. Schr.* p. 394.) has related some variations of his experiments. He found that water impregnated with acids, alkalis, neutral salts, expressed juices of vegetables, as of raisins, peaches, &c. very much promoted the production of pure air by vegetables, except in the case of the grasses (of which the product is variable from undiscovered causes), of the conferva rivularis, and water-plants in general, which are killed by fixed air, and some others, when this acid is employed. The difference is so striking that it will be proper to select some instances.

	Quantity.	Quality.
$3\frac{3}{4}$ cubic inches of a triplex lacini- ata in common water . . .	3	228
————— in strongly aërated water	$9\frac{1}{2}$	286
but $\frac{1}{2}$ was fixed air from the water.		
3 cubic inches of yew, , -	4	206
————— in aërated water	19	244
of which the pure air was	12	
2 cubic inches of grafs in 86 of common water	5	280
————— in 86 of aërated water .	13	336
N. B. $\frac{1}{6}$ was fixed air.		
2 cubic inches of yew in com- mon water	$2\frac{3}{4}$	225
————— in aërated water	20	322
$\frac{1}{3}$ fixed air.		

* In 6 hours of pretty fair weather.

	Quantity of Fixed Pure Air. Air.		Quality.
3 cubic inches of yew in 120 of common water . . .	$2\frac{1}{2}$	0	234
Ditto with the addition of			
2 cubic inches of must .	5	a little	270
with 2 of peach juice	5	a little	292
with 2 of ripe apple .	$3\frac{1}{2}$	$\frac{1}{2}$	250
with 50 drops of aqua- fortis	$4\frac{1}{2}$	$\frac{1}{4}$	250

* * 6 hours of bright sun-shine.

3 cubic inches of grafs in 86			
cubic inches of water .	4	0	248
I			2 cubic

	Quantity of Fixed Pure Air. Air.		Quality.
2 cubic inches of grafs with 1 cubic inch of must . .	9 $\frac{1}{2}$	a little	312
Ditto of ripe cucumber juice	2 $\frac{3}{4}$	a little	230
40 drops of vitriolic acid	7 $\frac{1}{2}$	$\frac{1}{8}$	295
a little vitriolated tartar	12	$\frac{1}{4}$	296

* * * fine sun-shine, 6 hours.

2 cubic inches of sempervi- vum tectorium in 86 of water	3	scarce any	245
Ditto with 1 cubic inch of its own juice	4 $\frac{3}{4}$	o	308

* * * * fine weather, 6 hours.

2 cubic inches of grafs in 86 of water	3	scarce any	245
Ditto with $\frac{1}{2}$ cubic inch of strong vinegar	7 $\frac{1}{2}$	$\frac{1}{8}$	276
with 1 of juice of the <i>cornus mascula</i>	8 $\frac{1}{4}$	$\frac{1}{8}$	323
1 of juice of onion	2 $\frac{3}{4}$	$\frac{1}{3}$	228
1 of juice of turnep	2 $\frac{1}{4}$	$\frac{1}{4}$	217

In these numerous instances, the effect of the addition equally appears from the numbers that express the quantity and those that express the quality of the air produced. It may be observed also, that a few substances, such as the juice of onions, cucumbers, and turneps, prevent, instead of forwarding, the extrication of air, and that it is of an inferior quality to that which is produced when no addition is made.

In the first place it is evident, that among the substances which favour the extrication of pure air, we have every thing which can well be supposed to enter into the composition of manure, salts simple and compound, with the juices and extractive matter of plants. If we may likewise assume, that the production of this salutary fluid is a natural function and an healthy process, it follows directly, that the use of manure is to occasion a greater exertion of that function.

That the production of dephlogisticated air is among the chief functions of vegetables is a supposition countenanced by many experiments. Mr. Cavendish himself infers, that the vital air obtained by Dr. Ingenhoufz comes from the decomposition of water. There is one experiment related by the last-mentioned author, highly remarkable, and not to be explained on any other supposition that has been hitherto thrown out. ‘ I boiled, (says he l. c. p. 198. B. 2.) some water for two hours, and then poured it boiling into a glass balloon of the capacity of 200 cubic inches. The balloon was then carefully closed. Before the water was grown quite cold, I introduced into the balloon four cubic inches of granulated green matter, which was taken out of the great reservoir in the botanic garden (at Vienna), and repeatedly washed in boiling water; care being taken to squeeze out after each washing all the moisture, in order that none except boiling water might remain adhering to it. I next closed the balloon with a perforated stopple, in order to allow the water an exit when it should be pressed by the air evolved from the green matter. The balloon was inverted into a vessel of quicksilver placed in the

the sun. The air generated at first was absorbed by the water itself: but being soon saturated, it refused to take up any more; and in the course of a few days I found a considerable quantity of air collected.' If it then be true that water is decomposed by vegetables, it follows that inflammable air is absorbed and fixed; an opinion countenanced by Priestley's experiments of charcoal, on sliced roots of onions, &c. for neither he nor Senebier, nor Ingenhoufz, nor I think any other, has ever found inflammable air in the elastic fluid afforded by the leaves and other proper parts of vegetables exposed to the sun. The inflammable matter which surrounds certain vegetables is, I suppose, an essential oil in the state of vapour; and if any plant should yield inflammable air in the way I have mentioned, I doubt not but it would furnish, in other respects as well as in this, a singular exception to the rest of the kingdom.

There is still another corollary more precise and satisfactory to be drawn from these premises. The quantity of dephlogisticated air that is extricated, will afford a test of the quantity of food taken in by the plant.

It may, moreover, be supposed, that the additions, which Dr. Ingenhoufz made use of, are not those which will produce the greatest effect. It is reasonable to suppose, that Nature in the immensity of her stores, has stimulants far exceeding these in power; which further inquiry will both discover and teach how to apply. For if these principles be just, they will be easily applicable, when we are in possession of a greater number of facts, both to
gardening

gardening and agriculture : And I doubt not but that in time a rational system of vegetable medicine may be constructed, if the subject be properly prosecuted. In the mean time, languishing trees may be washed or sprinkled with water acidulated with vitriolic acid, which Ingenhoufz found to be most effectual in promoting the production of pure air.

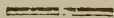
It will not be difficult for any person who may choose to reflect on the subject, to contrive other experiments, by which these principles may be confirmed to refuted.

THE END.

POSTSCRIPT.

I KNOW how much MILLMAN insists upon the crassamentum, observed by some authors in scorbutic blood. But in such a case I think a very few *negative* more than counterbalance thousands of *positive* testimonies, where no account is given of circumstances, and no attention has been probably paid to them: the very mode of blood-letting, the time it stands before it is examined, the temperature in which it is kept, &c. may affect the combination of blood with oxygene, and by consequence, its coagulation.

It is perhaps, in the present improved state of chemistry, practicable to ascertain the laws of the coagulation of the blood: M. PARMENTIER and DEYEUX, have not indeed perfectly succeeded in their researches on milk, a very similar fluid. It is remarkable, that in their experiments the heat of boiling water would not curdle or produce a skin upon milk without the presence of air, and yet that it should have been indifferent what kind of air was present.



N. B. I have lately attended to the colour of phthical blood; and in some instances where it was just drawn, I have perceived both the *florid* and *claret* or *purple* colour; the former is distinctly seen, when the blood is spread thin, the latter, when it has a considerable depth. This will explain the apparent contradiction in p. 124.

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